Lumleian Lectures

ON LIFE AND ON VITAL ACTION IN HEALTH AND DISEASE.

Delivered at the Royal College of Physicians,

BY LIONEL S. BEALE, M.A., F.R.S.,
PHYSICIAN TO KING'S COLLEGE HOSPITAL.

LECTURE III.—PART I.

CONSTRUCTION BY BIOPLASM (CONTINUED).

MR. PRESIDENT AND GENTLEMEN,—One of the most remarkable examples of inherited constructive power, and one which, as it seems to me, is especially worthy of our consideration with reference to many abstruse questions relating to the transmission of vast power through the agency of infinitesimal particles, is the bioplasm of the spermatic particle. The powers derived from the organism which produced it are by this tiny speck of living matter transferred to bioplasm through which its influence is brought to bear upon vast quantities of living matter.

The bioplasm of a human spermatozoon, which, perhaps, hardly weighs as much as the one-hundredth part of a single red blood-corpuscle, may stamp with unmistakable individual characteristics several tissues and organs which, in their fully developed state, weigh many pounds. Moreover, the influence of the power thus conveyed extends over many years, and is as distinctly manifest in the action as in the structure of tissues. This influence may not be clearly evident until many years have passed since the minute particle of bioplasm left the organism that produced it.

But how are we to explain the association of such far-reaching constructive power with molecules of such minute size? With what properties of ordinary matter can these powers be placed in comparison?

The special particles of matter endowed with the powers under consideration are produced in one particular organ, which does not attain its power of production until many years have passed since its development commenced. Surely no one would consider it reasonable to attribute the powers manifested by the spermatozoon to the properties of the elements of which its minute speck of bioplasm is composed. No properties of any elements that have been discovered are in any way comparable with those manifested by this wonderful particle. Mr. Darwin has therefore endeavoured to explain the marvellous properties in another way. But let us consider what insuperable objections immediately present themselves to the acceptance of his hypothesis of pangenesis.

Here is one of the very smallest living particles carrying the most extensive powers—powers which may reach to every tissue in the being that is to be formed. It is nevertheless suggested that the spermatozoon may be composed of millions of particles, one or more having been detached from every component element of the tissues of the parental organism. But this is not all; for every one of millions of spermatozoa must be considered as having been formed in the same way.

Now concerning the manner in which the supposed particles meet together to form the spermatozoon. We are asked to conceive that particles are being continually detached from each anatomical element of each tissue, and that they pass from every part of the body where they were formed, and move with unerring precision towards the gland-tubes, where they collect together in sets without confusion, thus to form the spermatic particle. If several particles from the same locality come together, they must separate again, and find their way to the collections which were deficient in these particular particles. By what means all these millions of particles find their way to the precise spot which is their proper destination; how they sort themselves, or by what means they are sorted; how at last they arrange themselves in proper order in the minute particle of bioplasm which they are suppose to constitute,—has not even been suggested. But this is not all. The hypothesis assumes that particles are detached from each anatomical element. But from what *part* of the cells they are supposed to come is not stated. Now, these anatomical elements are complex in structure, and many are composed of several kinds of matter. Many are growing, and the matter of which they consist is in very different states in different parts at different periods. As I have shown, the outer parts of the cells and certain of their contents are in many cases non-living. Now, if non-living particles were to collect in the manner assumed, the resulting collection would not be anything like a spermatozoon; and it is not conceivable that such a collection of non-living particles could in any way have vital powers communicated to them after they had collected together. On the other hand, if we assume that the minute particles from the different anatomical elements are detached from the bioplasm, our difficulties will still be found to be insuperable; for the bioplasm is surrounded by the formed material, and we should have to assume that minute particles of bioplasm passed through even the thickest and hardest formed material in a direction the opposite of that in which nutrient fluid was flowing. We must further conceive such living particles as making their way into the intercellular spaces, and directing themselves or being drawn into the bloodvessels or lymphatics, or working their way through the tissues in the most direct course towards the seat of their development. By what means they find their way, why they do not take up nutrient material and grow so large as to be stopped in their course, how it is that many are not lost and impacted in growing parts, it is impossible to conjecture. Upon the whole I think it will be difficult for anyone to accept this doctrine if he tries to realise the occurrence of the phenomena implied by the terms of the hypothesis.

Lastly, I have to consider whether it is probable that in living things, especially those having an organism of complex structure, there is any agency which exerts *a directive influence* over the whole, as some have supposed. It has been suggested that a general directing agency might be influential in regulating and governing the phenomena contemporaneously proceeding in all parts of a living body which result in the formation of very different structures—somewhat as a number of workmen are controlled and directed by a central authority placed over them. And, certainly, the facts of the case do seem to demand some such postulate; for, as each part grows and undergoes development, changes occur in other parts not anatomically connected with the first, but otherwise related, though separated perhaps by a considerable distance. But the more carefully we consider the question the less shall we feel inclined to accept any hypothesis which involves the idea of supervision on the part of any central power acting in the organism. There is, in fact, no centre from which such power could act, and there are no means by which it could be conceived to operate upon tissues at a distance. I cannot even admit that any facts known to us justify the belief that one cell is capable of acting upon adjacent cells. I do not believe that any particle of living matter is capable of influencing the phenomena of any other particle except, perhaps, by taking up more than its own share of pabulum, in consequence of an unequal distribution. I have elsewhere adduced reasons for rejecting Virchow's views upon this matter. It is difficult to conceive how one living particle can "excite" increased or diminished action upon the part of adjacent living particles; and I cannot think that under any circumstances do particles of living matter act in sympathy or in antagonism with neighbouring particles.

It will be asked—How, then, are the facts to be explained? If the development of any particular tissue be carefully studied, as may be done by comparing specimens taken at different stages of growth, the observer will be able to form a conception of the phenomena which succeed one another in an orderly manner, until, in place of the bioplasm particles of the embryo, he finds a highly elaborate arrangement of parts performing a definite office; but I fear I shall fail in my attempt adequately to represent the series of pictures which would be formed and combined into a moving panorama in the mind of one who had thus studied.

At first, all that is to be seen is a number of masses of bioplasm which divide and subdivide. Each bioplast, by virtue of its inherent vitality, continues to produce successors in regular series, and at a certain rate,—and a

limited range of change in external circumstances, being, as it were, allowed for, does not affect the result.

After the process of multiplication has continued for a certain period of time, each portion of bioplasm undergoes change upon the surface, and produces formed material. The arrangement and shape assumed by each particle will in some measure be determined by the growth of neighbouring masses and the pressure that may be exerted, as well as by a number of other circumstances. Indeed, the general form of the organ or tissue is determined chiefly by the inherent vital powers of each individual portion of bioplasm out of which it is formed, but in part by the growth of contiguous organs or structures. Whether the elementary part shall be drawn out like tendon, muscle, or nerve, flattened, many-sided, or oval, like certain forms of epithelium, will depend upon its connexions which were determined at a very early period, and whether it be subjected to stretching or pressure. If free to grow equally in all directions, as in fluid, the mass will certainly be spherical. Many questions, however, that might be asked with reference to many of the matters to which I have very briefly adverted cannot, in the present state of our knowledge, be answered, and are likely to remain unanswered for a long period of time. But I venture to think that if scientific men substitute conjectures for answers, and confidently recommend to the public their own speculations as if they were scientific conclusions based upon evidence, it is the duty of other scientific men to examine the propositions, and to carefully criticise them. From what has lately fallen from some of those attached to the doctrine of evolution, it would almost seem as if they considered that their views were to be accepted by us without even the slightest examination. Against this idea one cannot too strongly protest, and especially when one finds the most far-fetched hypotheses advanced and pressed upon the people as worthy of being accepted and believed as actual truths. No objection ought to be made to the free discussion whether, for instance, certain instincts constitute the foundation of the moral faculties, but it would be monstrous to assert that such had actually been proved to be the case. In the same way, attempts might be made to force people who have not the knowledge to enable them to form a judgment upon such a question to believe in the truth of the hypothesis that man has resulted by direct descent from lower forms. Now one can conceive that if but one clear case could be made out in which it had been clearly proved that two distinct species of any kind of animals had descended from a common progenitor, the evolution hypothesis would be received almost without opposition; but at present no evidence advanced is perfectly conclusive, and people ought to be allowed to discuss the matter freely without condemnation. "Aut Darwin aut nullus" is a method of argument that seems to me by no means more convincing than the assurance on the part of a philosopher that he would prefer to have descended from an amiable beast than from a savage man.

BIOPLASM IN DISEASE, AND THE DEGRADATION OF BIOPLASM.

Having considered the changes taking place in simple living matter, and those wonderful phenomena which succeed one another with perfect regularity and in perfect order, until the formation of the organism, with its complex tissues and organs, is accomplished, let me ask your attention to vital phenomena of a different kind, which result in degradation as regards formative power, although there is *increase*, not diminution, of those changes to which I would restrict the term *vital*. We can trace the formation of the living disease-particle from normal living matter, and may see exactly how it results from an alteration of the conditions under which the life of normal bioplasm is carried on. Not only so, but, if I have interpreted the observations aright, I shall be able to show how the disease-carrying germ is produced in the living body as a consequence of abnormal changes going on in successive generations of bioplasm particles; these morbid living particles having invariably originated, as I believe, in normal bioplasm.

If the bioplasm of man or one of the higher animals grows more rapidly than in the normal state, its power of forming tissue and of developing structure is impaired. If any structure whatever is produced, it is soft, incapable of discharging its function properly; it is weak, and it does not last. It has been produced too quickly to attain power of resistance. But if the rapidity of the growth of bioplasm is very considerably increased, no tissue at all results. Bioplasm produced by descent from that which grows and multiplies quickly does not regain formative power. Rapid increase is associated with loss in constructive power, but, as I have remarked, *vital activity* is increased, if by the phrase we mean to imply that more pabulum undergoes conversion into bioplasm, within a given time, than in the normal state; and I know no other meaning applicable to the words.

If the bioplasm of any tissue in a state of inflammation or fever be compared with that of the same tissue in health, it will be found that the bioplasm masses have considerably increased in size. In every kind of inflammation and in all ordinary fever bioplasm grows. It is, in fact, the increase of the bioplasm that constitutes the malady, and gives rise to the characteristic phenomena of the disease. I shall not describe the changes as they may be observed in the tissues, for I have already considered this part of the subject in my Report on the Cattle Plague, published in 1866, in "Disease Germs," and a brief account has been given in one or two other works. The essential change, both in inflammation and fever, seems to be *increased nutrition of bioplasm*. An inflammation, as I have remarked, may be regarded as a local or circumscribed fever, and a fever as a general inflammation. The growth of the bioplasm in ordinary fevers does not proceed far enough to end in pus-formation, because the general changes in the blood, in the nervous system and in other parts of the body, cut short life long before the whole organism could pass into a state of general suppuration; but how often do we find a sufficiently marked tendency to such a state in the extensive and too often widespread suppurations occurring in tissues and organs of persons who just escape death from severe attacks.

Now, what is the cause of the increased nutrition and growth of the bioplasm? In fevers and in every form of inflammation the normal phenomena in the bioplasm undergo a striking change. The bioplasm of healthy tissues, which is living very slowly, taking up little nutrient matter, and slowly being converted into formed material, perhaps gradually diminishing, but never increasing in size—suddenly, quickly, that is perhaps within a few hours, increases greatly in amount. It may divide and subdivide into several masses, all of which grow rapidly and take up a large quantity of nutrient matter. So far from the production of formed material of tissues going on under these circumstances, not only is this change for the time suspended, but much of the formed material last produced, and still in a soft and imperfectly developed condition, is taken up by the bioplasm which produced it, and the latter, in fact, grows at the expense of the structure of the body. Much tissue may in this way be destroyed, and portions of organs completely removed, never to be replaced. The body is thus permanently damaged, for the parts destroyed could not be reformed, unless they were developed anew, and this in man and the higher organisms is impossible. The power of new formation, however, does exist, and to a wonderful extent, in many of the lower animals, the fully-formed lobster being able to develop a new claw, the salamander and axolotl new limbs, and the lizard a new tail. The muscles, nerve-fibres, vessels, and all the complex tissues entering into the formation of these organs, be it observed, are developed from formless bioplasm, in the same manner as they are formed during the embryonic period of existence. Man cannot develop a new finger—not even a nail or a tooth—if these structures, with the matrix from which they grow, be removed after their development is completed. Not even a bone will be replaced if, with the bone itself, all the soft tissue which takes part in bone formation be removed. But new vessels, new nerves, new epithelium, new fibrous and some other tissues may be developed even in man long after he has reached maturity. Vessels and nerve-fibres are, however, formed in continuity with and from the bioplasm of already existing tissues of the same kind. A low form of epithelium and connective tissue, with the exception of certain textures found occasionally in the ovary, are probably the only tissues which are formed in the adult from bioplasm, which we can be sure has not been derived from that belonging to the tissues. The above may be, I believe, developed from bioplasm which has been produced by the white corpuscles of the blood.

I must, however, leave this interesting question, and revert to the consideration of the cause of the enlargement of the masses of bioplasm in fever and inflammation, but we may first inquire what would happen if the living matter did *not* increase—if pus were *not formed*, and perhaps much tissue destroyed? Although the inflammatory process in question is undoubtedly destructive, the tissue destroyed is as nothing compared with the amount of texture that would be totally destroyed by decomposition if the phenomena of inflammation did not occur. The bioplasm particles would die, and that regular flow of fluid to and fro in the very substance of all the tissues, being deranged, the bioplasts near the part affected would also die, and the organic matter of these, with that of the tissue, and organic matter present would undergo decomposition. The resulting products would diffuse into the neighbourhood, and extensive mortification would ensue. The blood would stagnate in the neighbouring vessels, its bioplasm would die and undergo decomposition. In such cases it too often happens that the death of the organism very soon follows. If, however, a part of the body mortifies, and inflammation occurs around the dead tissue, life may be saved, and the dead mass detached. But in this case there must be rapid growth and multiplication of bioplasm between the part that is dead and the healthy tissues.

If the bioplasm of any tissue be supplied with more nutrient material than it receives in the normal condition, it will grow more rapidly than usual. This increased growth, owing to an increased access of nutrient pabulum, is the *first change that occurs in inflammation, and it is essential to the inflammatory process.*

The more ready access of nutriment may be due to an opening in the formed material and exposure of the bioplasm at one point, as may occur in mechanical injury; or, lastly, it may depend upon a softened state of the formed material which is thereby rendered more permeable to the fluids, or the character of the nutrient fluid may be so altered as to permeate the formed material much more readily than the nutrient fluid which transudes through the walls of the vessels from healthy blood,—a condition which is met with in some forms of inflammation, and in all the more severe forms of fever. In the latter state changes in the blood often proceed to such an extent as to materially modify its chemical composition.*

If we are to ascertain the true cause of the enlargement of masses of bioplasm in inflammation and fever, it is clear we must carry our inquiry beyond the fact of the increased access of pabulum, for it is obvious that the access of pabulum could not determine increased growth unless there was an active tendency on the part of the bioplasm itself to appropriate the pabulum and to grow—in fact, a tendency on the part of the bioplasm to move towards the pabulum, take it into itself, and convert it into living matter. Bioplasm *always tends to grow.* It *must*, in fact, grow if it be supplied with nutriment.

Bioplasm has within itself no power of regulating or controlling the rate of its growth. As already stated, the conditions under which it grows exert an important influence in the results of growth and development of bioplasm, and these "conditions" vary as regards every kind of bioplasm, and not only so, but different forms of bioplasm differ greatly in their power of bearing a change in the conditions without suffering deterioration. Some forms will live under external circumstances of the most varied kind, and are, therefore, found over a very wide area. Others suffer or are destroyed if the external conditions are altered only so slightly as to be imperceptible to many living creatures, and in this case the distribution of the life-forms, resulting from the development of the bioplasm, will be confined to a very limited area. The same observations may be made with respect to the several different forms of bioplasm in one living organism. Some, like that of epithelium, bear very great changes in temperature, &c., without being much affected, while other forms, like those of some glands and those of some parts of the nervous system, suffer under comparatively slight alterations of the ordinary conditions under which they live.† There is no cogent reason for supposing that all forms of bioplasm might be made to bear, without suffering, similar great alterations in external conditions, than for believing that we might domesticate the wolf and the lion, as we have domesticated the dog and the cat, or make the swallow flourish in our little home prisons like the canary bird.

As it were between the two extremes, are forms of bioplasm that grow and deteriorate, but still live for years under adverse circumstances, the structures formed never attaining a state of vigour, but still being formed and acting in an imperfect way, and perhaps for a long period of time. The weak, succulent, too quickly developed vegetation of our fern cases is an example; and the soft rickety tissues of some of our weak, flabby, over-fed, town-bred, highly precocious children, supply a very painful instance of too quick formation and growth. In such cases the bioplasm has grown too fast, and tissue has been formed too quickly. Time has not been allowed for its condensation and strength, and for the acquisition of resisting and lasting properties. But is it not a mistake to point to such cases as examples of diminished *vital action?* It seems to me that life has been carried on too fast and not too slowly. Too much pabulum has been taken up—not too little. Too much heat, too much food, favour a quick, rank, succulent, spongy sort of development, inducing the formation of soft bones and weak imperfectly acting tissues which are likely soon to deteriorate.

In connexion with the question of the nature of *vital* actions, the loss of formative power as the rate of growth and multiplication of the bioplasm increases must be regarded as a fact of great interest. Bioplasm may form tissue and give origin to multitudes of bioplasts, from which tissue may also be developed; but the very particle of bioplasm which might have taken part in tissue-formation will, if it grows and multiplies too fast, not only lose its power of forming tissue, but the particles that may be produced from it by descent will never regain the wonderful capacity that has been lost. No pus-corpuscle or any of its descendants, then, ever take part in tissue-formation. It has deteriorated, and the deterioration in formative power remains a characteristic of all its descendants. But although formative power is lost, new powers or properties may be nevertheless acquired. These are, however, remarkable for destruction, for pulling down and destroying that which has been formed, never for construction. Some forms of pus acquire during their production the most wonderful capacity for rapidly growing and multiplying, as well as for living and resisting the influence of external conditions. The little offsets or particles that are detached from them may rise in the air, live for a time in water or milk, or other fluids containing organic matter, adhere to a sponge or a probe or other substance, or be carried in a living state on the foot of a fly or some other insect, and thus transported to an organism at a distance from the one which was the seat of their production. The minute germs, being in contact with material adapted for their nourishment, rapidly grow and multiply in their new situation. As we know but too well, such forms of *virus* have been produced *ab initio*, fostered, and propagated to the destruction of hundreds of human beings.

In some cases we are almost able to watch the series of phenomena which result in the development of a contagious animal poison. I have shown that the active agent is a minute particle of living bioplasm, which I have good reason to believe may have been produced by descent from the normal bioplasm of the body. This particle of deteriorated bioplasm, which in some cases may be *seen*, is a "disease-germ," and of these "contagium" consists. The word *contagion* was used by Hippocrates, by whom, as well as by Harvey, and, indeed, by authors who lived since Harvey's day, it was employed to signify something immaterial, something intangible, that was supposed to emanate somehow from the affected organism. Now, however, we use this word in a very different sense, for by it we mean actual matter of some kind. This matter, as far as I have been able to ascertain, is composed of minute particles of bioplasm, which resemble in their general characters bioplasm which exists in some situations in the healthy body. The contagious bioplasm results from normal bioplasm, the life of which has been carried on for some time under unusual conditions. It has been already shown that an ordinary form of morbid bioplasm, pus, often originates in

* "Disease Germs," p. 340.

† The "conditions" under which bioplasm grows in the higher animals are, it must be borne in mind, very complex and are determined partly by the results of growth of pre-existing bioplasm and in part only by external conditions. The same remark applies to the "environment" of any cell or elementary part of the body.

the bioplasm of epithelium and in that of connective tissue, and that certain forms of pus have specific virulent properties, and are, in fact, animal poisons, which may be inoculated.

I do not, however, entertain the opinion that all contagious disease germs spring from the bioplasm of epithelium or connective tissue. Some may come from white blood-corpuscles, or from lymph-corpuscles. But I do consider that many facts favour the doctrine that the contagious particles concerned in propagating many of our most serious specific fevers have been derived from the living matter of man's body, and that they are not germs of fungi, or bacteria of any kind whatever. Indeed, in certain instances they may be seen in far greater multitudes, in the tissues of the diseased organism, than bacteria, which latter, as is well known, are found often enough in countless multitudes in cases in which there is no specific disease of any kind.

When I was studying the changes in the tissues of animals destroyed by cattle plague, in 1865, I found a quantity of rapidly growing bioplasm in almost all the tissues, but in the skin the manner in which the contagious particles grew and spread could be very satisfactorily demonstrated. The minute particles, having gained access to the interstices between the bundles of fibrous tissues of the corium, divided and subdivided, spreading amongst the bundles and separating them from one another. In some places the particles had made their way into the interstices of the epithelial cells of the cuticle, where they formed little collections, and had invaded the tissue itself of the cuticle cell. Similar bioplasm particles were found in the obstructed capillary vessels, and in not a few instances could the course pursued by these be traced from the interior of the capillary to the surrounding tissue by a narrow line of well-defined bioplasm particles. Although it is true bacteria were to be seen in some situations, no difficulty was experienced in finding collections of the bioplasm quite free from bacteroid bodies or their germs—a fact which I think conclusively proves that there is no necessary connexion between the specific bioplasm particles and bacteria.

I have for years past—in fact, ever since 1861—directed attention to what I cannot but characterise as the many insuperable objections to the acceptance of the bacterium and fungus-germ hypothesis of contagious fevers. In these days there is nothing more striking than the pertinacity displayed in forcing upon our attention extravagant conjectures which seem to be almost unsupported by facts. One great scientific authority tries to persuade us to go along with Lucretius, another seems to think that it is our bounden duty to believe and confess that we are machines. We are expected to acknowledge that there are forms of life the physical actions of which are not far removed from those of an oil-globule, that between a piece of lifeless white of egg and living bioplasm there is only a slight difference, and that omnipresent bacteria, which it is gravely asserted may be made out of non-living matter in a glass flask or allied forms, are the active agents, or at least the essential concomitants, of almost every form of contagious disease which can be imparted by an infected to an uninfected organism. Indeed, there is no end to the suggestions advanced to gratify our love of the improbable, which are made to appear plausible by the selection and ingenious application of a few arguments, while the numerous facts opposed to them are altogether ignored.

Some writers have committed themselves to the fungus-germ theory of disease, and have so clearly stated their views that it is scarcely possible to misinterpret their meaning. Others have expressed themselves against this doctrine with equal clearness and equal force. But there is a third class of persons who seem to think that fungi may or may not be the active *materies morbi*. If, say they, contagium does not consist of actual bacteria it is caused by bacteria, or bacteria constitute an essential condition of propagation, or they prepare the body for the reception of the contagium, or they make the pabulum upon which the contagium lives, or they are a stage in the development of contagium, or they are constant and necessary accompaniments of contagious particles. All this in spite of the broad fact staring us in the face that at the time when certain contagious matter is most virulent, no bacteria, or few or doubtful bacteria, are to be discerned; while as the bacteria multiply the specific properties of the poison become weaker, and when the bacteria are growing rapidly and freely multiplying its specific properties have disappeared. Again it is assumed that there is a necessary connexion between the development of bacteria and the formation of pus, although anyone can satisfy himself by observation that the living moving pus-corpuscle is destitute of bacteria germs, while the dead inactive pus-corpuscle, or, more strictly, the matter resulting from the death of the pus-corpuscle, is being devoured by them.

The very last conclusion, it seems to me, that would be adopted by anyone who thoroughly thought over the matter would be, that these low organisms were the causes of the changes of the fluids by which their growth was favoured, much less that they were the causes of the diseases which had existed for some time before they began to multiply. One might, I think, with as much show of reason, advocate the doctrine that bacteria were invariably the cause of death, since after every form of death they grow and multiply in the tissues and fluids of the human body.

We have now examined a somewhat extensive range of vital phenomena, and nowhere have we discovered a change or an action peculiar to living matter that could be adequately accounted for by any known facts and laws. The formation of every tissue or organ seems to have been anticipated and prepared for, as it were, beforehand; and, in many cases, changes occur which it is difficult to persuade oneself were not in some sense foreseen, because provision is made for their occurrence long before the changes happen, and the preparation is such that it could only be accounted for upon such an assumption. But, in fact, this is characteristic of every kind of vital action, although it is much more apparent in some instances than in others. It need scarcely be said that physicists have failed to adduce any explanation of the phenomenon which, like many other facts to which I have adverted, is peculiar to life.

I must apologise for troubling you with so many remarks against any form of physical doctrine of life, but I shall trespass very little more upon your patience. If by physics and chemistry the phenomena common to all living—even in the case of the very lowest, simplest living—organisms cannot be adequately accounted for, it seems to me it would be a waste of time to recount the numberless facts and arguments against the acceptance of a physical doctrine as applied to thought. Nor shall I be greatly moved by the hardest words that may be used, or the intensity of the scorn that may be displayed, by those who regard the brain as a machine, and thought as a result of chemical change. They may be well supported in their views, but until they can give a rational account of the changes which occur in a simple living organism, no one who uses his reason aright will believe that they really discern the nature of the difference in the state of the nervous matter of man's brain when the organ is active and when it is in a state of rest.

Have the supporters of physical doctrines been able to tell us what goes on when a nerve grows, what occurs when it acts, or the difference between the nerve-matter of the nerve-centres and nerve-fibres when they are in a state of the highest activity and in most complete rest? Have they shown us how a single nerve-fibre is formed in its position, much less accounted for the manner in which that inextricable interlacement of fibres characteristic of all nerve texture is brought about? Have they proved how an odorous or a sapid particle influences the nerve, or the differences between the vibrations of the molecules of the nerves concerned in sight and hearing, or what happens when a nerve-centre receives an impression from the periphery, or transmits an impulse outwards—nay, have they advanced a plausible theory to explain how we are able to bring about a definite degree of shortening in muscular fibres, maintain it for a time, and vary it as we will? And yet we are expected to accept the dicta concerning the nature of the highest and most complex actions of man's organism, of authorities who are professedly unable to enlighten us as to the changes which occur when an amœba or a bacterium moves, or takes up nutrient matter, grows, or divides and multiplies.

DR. WILBERFORCE ARNOLD has been unanimously elected honorary secretary of the Belfast Branch of the Royal Medical Benevolent Fund Society of Ireland, in the room of Dr. Robert Stewart, deceased, who held the office for very many years with distinguished honour.

CASE OF LITHOTOMY.

By HAYNES WALTON, F.R.C.S.,
SURGEON TO ST. MARY'S HOSPITAL.

Inability to pass Urine naturally for Thirty-one Years, in consequence of mechanical injury to some part of the Urinary Apparatus; formation of Stone in the Bladder; Lithotomy; complete Recovery of the Tone of the Bladder.

MY patient is aged seventy-one. He consulted me in the spring of last year by the recommendation of Dr. Henderson, on account of far-advanced chronic catarrh of the bladder, and of the frequent necessity for passing urine.

His history is this:—Thirty-one years ago he fell into an area from the breaking down of a balcony. He was slightly bruised in several parts of his body; but thought nothing of his fall until he made water, when he found that his urine was bloody. The source of the blood cannot now be told. He bled for five days. Then followed dribbling of urine, if much urine were allowed to accumulate in the bladder; and shortly afterwards he could no longer pass his water naturally.

The medical attendant taught him to use a silver catheter, and directed him to employ it sufficiently often to relieve himself. Since that time he had been obliged to draw it off three or four times or more in the twenty-four hours. Till within five years the water was always clear. Then followed turbidity, and with it a desire to be relieved more frequently in the day, and also in the night. When I saw my patient he was catheterng himself hourly by day, and every two hours during the night. His sufferings, in consequence of soreness of the urethra and bladder irritation, were very severe.

I sounded the bladder and detected a stone. The prostate gland was enlarged. I operated in July, assisted by my colleague Mr. Norton, my house-surgeon Mr. Perkins, and some of my pupils, and removed a large phosphatic calculus. Nothing was noticed during the operation worthy of note, except it be the unusual hardness of the prostate. Recovery was uninterrupted and complete. The mucous membrane of the bladder gradually became normal; the bladder functions were restored; and at this time my patient has neither atony nor irritability of that organ. He can pass his urine when he likes, or retain it after he has had a desire to get rid of it, and never uses the catheter. He usually rises to micturate once in the night, but he passes several nights in a month without doing so.

Brook-street.

NOTE ON SULPHATE OF CHINCHONIDINE.

By SURGEON-MAJOR GEO. YEATES HUNTER,
PRESIDENCY SURGEON, BOMBAY.

GOVERNMENT, on the favourable report of Mr. Howard, quinine manufacturer, London, having ordered that the therapeutic value of sulphate of chinchonidine should be tested, I made a trial of it in the hospitals of the House of Correction and the County Gaol of Bombay, and the following short account embodies my two reports on the results of the experiment.

The drug was employed internally (*per viam naturalem*) for malarious fever, in doses of from five to ten grains, as well as hypodermically in a solution containing the following proportions:—Sulphate of chinchonidine, five grains; dilute sulphuric acid, five minims; water, ten minims.

From September, 1874, to February, 1875—a period of nearly six months, the term fixed upon for the experiment—I treated fifty-five cases of fever, chiefly intermittent of a mild form, due to sudden changes of weather, exposure to cold, privation, &c., or to indiscretions in diet. In the majority of these cases three days (equivalent to a consumption of ninety grains of the alkaloid at ten grains thrice daily) sufficed to effect a cure, or, perhaps I ought more correctly to say, to see an end of the slight attack. In some instances of a severer type (in men who came from malarious districts, for example) the chinchonidine completely failed to check the fever, and quinine had to be resorted to. In one bad case of remittent fever, which I regret to say ended fatally, chinchonidine was tried for two days, and discontinued on account of its proving ineffectual, thus strengthening my impression that it is worse than useless to depend upon this drug in severe fever set up by the poison of malaria. In a few cases its subcutaneous injection appeared to be of service, and I observed that no troublesome inflammation at the point of insertion followed its use.

I may mention that in nearly every case in which chinchonidine was administered, headache, almost amounting to semi-congestion of the brain, resulted, which accords with the experience of some who tested the therapeutic value of this alkaloid on a former occasion. To obviate the symptom just alluded to, it was the practice in my hospitals to combine one grain of opium with ten of the chinchonidine. Sickness at stomach was also a frequent symptom, more often than it was noticed from quinine, which latter is undoubtedly a cholagogue, whereas one of the effects of chinchonidine is to lock up the secretions.

I am of opinion that in some cases of a mild form of fever, for which ninety grains of chinchonidine were given, two or three grains of quinine twice or thrice daily for the same time would have stopped the fever. Perhaps it would be a fair representation of the case to say that ten grains of chinchonidine would be required to produce the same effect as three grains of quinine given in mild cases, and even then I am not sure that it would not be necessary to continue the treatment by the former longer.

I think the drug for its therapeutic value might be classed with quiniodine, liquor quiniæ amorph., chirettine, bebeerine, &c., and their effects as antiperiodics is doubtful. Certainly chinchonidine cannot be ranked as an antiperiodic with quinine, and is not a specific for fever in the sense that remedy is. It may be of service as a good tonic after fever has been controlled by means of quinine, and perhaps prevent relapses; it might also answer as a prophylactic to those living in, or travelling through, malarious districts; but I would never rely upon it in bad cases of malarious fever, although probably it might suffice to check it in old feverish subjects, in whom the disease had been relighted up by transitions of temperature, or by some disorder of the general health.

The conclusion I have arrived at is, that the drug is of greatly inferior value to quinine, and I cannot imagine under what circumstances it could be, as Mr. Howard suggests, preferable to quinine, even in the milder cases which practitioners at home are called on to treat. Were it of equal efficacy even, there would be no economy in its use, since much larger doses are required, which would involve an equal expenditure of money, to say nothing of the inexpediency of having to take three times the quantity of a medicine to produce the same effect. Should there be a slight saving on the whole, I think it would be dearly effected at the price of occasional failure in mild cases, and perhaps the loss of precious time when malarious fever of a bad type presented itself for treatment.

I see no advantage, therefore, in substituting this drug for quinine, not even from an economic point of view, which has really ceased to be a pressing consideration now that the hypodermic mode of practice is so widely adopted.

I may add that I tried the drug only in the very mildest cases, when experimenting with it in the Byculla schools, a charge devolving upon me as presidency surgeon, for the under-current of fear I had that dangerous delay might ensue in controlling the fever in bad cases applied with still greater force when little children were concerned.

I have not yet seen any reports on the subject, but no doubt other medical officers will make known the results of their experiments.

BEQUESTS, &c., TO MEDICAL CHARITIES. — Mr. George Stockdale, of Hanover-terrace, Notting-hill, bequeathed £500 to the British Home for Incurables, Clapham Rise. Miss Ann Coombe bequeathed £200 to the general funds, and £200 to the Samaritan Fund, of the General Hospital and Dispensary, Cheltenham. The Kidderminster Infirmary has received £500 under the will of Mr. Joseph Chellingsworth, of Trimpley.

THREE CASES OF

SUCCESSFUL REMOVAL OF FIBRO-CYSTIC TUMOURS OF THE UTERUS.

BY THOMAS KEITH, M.D., F.R.C.S.

CASE 1.—In December, 1873, Miss S——, aged fifty-two, consulted Dr. Perry, of Glasgow, on account of a tumour which she had just discovered in her abdomen. Dr. Perry pronounced it to be a small fibrous tumour of the uterus, and told her that it would probably follow the usual history of such growths at her time of life, and give rise to little or no inconvenience. Two months after this I expressed a similar hope, adding, however, as she dreaded some cancerous affection, that should her health and comfort come to be seriously interfered with, she could look forward to permanent relief by operation. The tumour grew rapidly, and in June, six months after it was noticed, reached above the umbilicus, and was distinctly cystic. Her general health was then wretched, but after two months in the country she improved in strength, and became anxious to have something done before winter, for she had long suffered from bronchitis, and was generally confined to the house during cold weather. Having objections to removing small tumours, I put her off from month to month. She made her appearance in the end of October last, suffering then from cough and bronchial catarrh.

The patient was fair-complexioned, anæmic, extremely emaciated and fragile-looking. She was the youngest of a family of twelve, and had never been strong. At sixteen she had severe pleurisy, on account of which the right side of the chest remained contracted. It was much flattened, and the edges of the ribs were depressed towards the spine, thus interfering to a great degree with the capacity of the abdomen. The tumour touched the edges of the ribs on the right side, and there was now a considerable amount of ascitic fluid. The abdominal wall was very thin; fluctuation was distinct in the right side, where there was a prominent cyst the size of the head. Part of the tumour came down into Douglas's space; it was there cystic and non-adherent. The cervix uteri was flattened, not enlarged, central, and rather low in the pelvis. Movements of the tumour affected it slightly, just as they affect the cervix in most ovarian tumours surrounded by free fluid. No specially careful examination was made; a single, almost momentary, feeling of the pelvis sufficed, for now I never doubted that the tumour was other than ovarian. It was looked upon as one of those common cases where there had been an escape of fluid from some of the cysts into the cavity of the abdomen, and in consequence chronic peritonitis had been set up. In such cases my practice has for a long time been to advise earlier operation than in cysts not accompanied by free fluid; for experience had taught me that, if often tapped, such patients do badly after operation. Besides, I had now an additional reason for early removal of such tumours, for the results of observations made for some time by Dr. Foulis showed that secondary epithelial growths on the peritoneum may originate from escaped epithelial elements from burst cysts, and set up exhaustive peritonitis.*

On the 2nd of November last sulphuric ether was given, the tumour exposed, the ascitic fluid allowed to escape, and the large prominent cyst punctured. Four or five pints of bloody serum slowly escaped. Then by pressure more fluid was got away, the opening enlarged to admit the hand, and the cystic portion in the pelvis somewhat broken up. It was then noticed that the fluid from the cysts coagulated on exposure to the air, and it was evident that the tumour was uterine. The wound was enlarged, and the tumour turned out, along with the left ovary, which was the size of a hen's egg. The uterus spread out rapidly into the cystic mass, the neck being the thickness of the arm, too large to be embraced by any clamp. After freeing the bladder downwards, a steel wire was secured round the lowest part of the neck, just above the vagina, by Koeberlé's instrument. The right ovary was healthy, and was not removed. The tumour was then cut away, and as the stump was pared down as close to the wire as was deemed safe, the cavity of the uterus was opened. It contained a small polypus, and its length was probably less than normal. The wound was closed tightly round the neck, a strong soft needle bent in the middle securing the whole outside. Though the abdominal wall was thin and relaxed, there was tension and depression of the stump towards the sacrum. The growth was a true fibro-cystic tumour of the uterus, and weighed 11 lb.

The patient recovered, and returned to Glasgow thirty-seven days after operation.

CASE 2.—An unmarried woman, aged forty-four, was admitted in February, 1874, into Ward 16 of the Royal Infirmary, where I saw her at the request of Dr. Matthews Duncan. She was a pale, thin, unhealthy-looking woman. She had granular everted eyelids, and was half-blind from old inflammations of the cornea. In the neck were cicatrices of suppurated glands. Up till June last her health had been fairly good; she was then obliged to give up her situation as cook in London, where she had lived for more than twenty years. Menstruation had always been regular and normal. Five weeks before admission a tumour had been detected; it was hard, elastic, quite fixed, and reached to the umbilicus. The cervix uteri was drawn to the left side of the pelvis; it was almost beyond reach of the finger, and felt as if lost in the tumour. No doubt was entertained that the case was one of uterine fibroid. After two months' residence in hospital she was dismissed, all interference being declined.

In the course of the summer she wrote several times to say that she had suffered severely, and that the tumour had greatly increased in size. She was unable to do anything for herself, and was anxious to have something tried. Though told that hers was not a case for operation, she came back in the beginning of September. The tumour had increased; it was tender, and had become very prominent on the right side, pushing the loin outwards. The feeling of elasticity was less marked, while that of fluctuation was pretty distinct, and there was some ascitic fluid. The relations in the pelvis were the same, the tumour filling the whole upper pelvis. Notwithstanding the presence of ascitic fluid, the tumour felt everywhere fixed and immovable. On the 5th of September a puncture was made at the umbilicus; the fluid removed was of a dark-brown colour, depositing on standing a little blood. It contained blood-corpuscles, white blood-cells in large numbers, and some cells resembling pus. Much irritation followed this tapping, and the ascitic fluid increased. After ten days the cyst had become tense, when sixty ounces of the same kind of fluid were withdrawn. The pain was relieved by this, and she was again sent away.

She soon came back, importunate for operation at any risk; her life was miserable from pain, and her health had now given way. The cyst was again tense, and the whole tumour was growing rapidly. It was agreed to make an attempt to remove it, the patient clearly understanding that this might not be accomplished.

On Dec. 12th the tumour was exposed by a free incision. It was of a dusky brown colour, covered by enormous veins. It was firmly attached to the right iliac fossa, right lumbar region, and partly to the abdominal wall. This extent of adhesion quite accounted for the fixed state which the tumour had always presented. The hand could not be passed into the pelvis below the tumour. Upwards of sixty ounces of dirty reddish serum were removed from the large cyst, without much diminishing the size. The hand was then passed behind, and with one strong sweep of the arm the whole adhesions were broken up, the incision enlarged, and the tumour turned out of the abdomen, along with the left ovary. In a few minutes she became quite faint, so rapidly was blood lost from the large extent of bleeding surface of the tumour. The pedicle—that is, the neck of the uterus—was treated as in the former case. The broad ligaments were very vascular, but only the left ovary was included in the ligature. From the previous elevation of the cervix, the stump was secured in the lower angle of the wound with less tension than in the previous case. Many bleeding points high up near the posterior margin of the liver, where the adhesions commenced, were secured with much difficulty. The first part of the operation occupied only a few minutes, but it was upwards of two hours ere the wound was closed.

* Edin. Med. Journ., Feb. 1875.

A glass drainage-tube was left, passing down to the lowest point in the pelvis. The patient was pulseless when placed in bed, and the pulsation did not return in the left radial for upwards of half an hour. This was an anxious operation on account of the unusual loss of blood.

It is unnecessary to give here details of the slow convalescence. The red serum was removed from the pelvis every few hours; the whole amount within the first three days did not exceed three ounces. This the patient might easily have absorbed. During the second and third weeks flatulent distension was great, and there were fears of obstructed intestine. The canal of lymph, which after a few days forms round the drainage-tube, seemed to have led to the adhesion of folds of intestine at angles, and thus became the occasion of much distress and of no small danger to the patient. As in the previous case, the slough extended beyond the wire, and a very large cavity was left on its separation, which had quite cicatrised in five weeks. Six weeks after operation she went home, and now writes that she is perfectly well in every way.

CASE 3.—This patient was forty years of age, and was sent to me by Dr. Millar, of Londonderry. The case was one of rapidly growing soft cystic fibroid, with pain and profuse hæmorrhages. A very accurate diagnosis was made. The uterus and both ovaries were removed, along with numerous cysts in the broad ligaments. Though quite recovered, the patient has not yet returned to Ireland. An account of this case will be given at some future time.

In the first of these three cases there was a mistake of diagnosis as to the nature of the tumour. It is the only one of 194 operations. Hitherto I had the good fortune to avoid cases of soft fibrous and fibro-cystic tumours of the uterus. But as it has happened to all who have performed ovariotomy many times to come down upon a fibro-cystic tumour of the uterus instead of an ovarian tumour, I knew that I could not always escape; and for many years I have never gone to perform ovariotomy without being at the same time prepared to remove the uterus if necessary. Fortunately in this case the surgical treatment required was the same, for large fibro-cystic tumours of the uterus often kill as rapidly as do ovarian tumours.

These are the only cases in which I have interfered with uterine tumours by abdominal section. They are, however, enough to satisfy me that the removal of an enlarged uterus and ovaries is an operation not to be lightly undertaken. Without the knowledge that ovariotomy has given me, the results would probably have been different. Even as it was, in the second case the hæmorrhage was so profuse that the patient was pulseless in a few minutes after the operation was begun. The personal attention afterwards necessary was, in each, greater than in half a dozen average cases of ovariotomy; while the third had such profuse secondary hæmorrhage seven days after operation, that had I not, by mere accident, been on the spot almost immediately after the alarm was given, the patient must have died in a few minutes.

During the last twelve or fourteen years, I need hardly say, a large number of uterine tumours of all kinds have passed through my hands; and the advice hitherto given has been to let them alone, either because interference was unnecessary or because their removal was too dangerous. Of course I do not allude to submucous tumours that could be reached by the vagina. I might have removed scores of pediculated fibroids which, surgically speaking, were very tempting; but in these, nothing but almost absolute safety of operation could have justified interference, and they were all declined, though I was often pressed hard to operate. The melancholy results that have hitherto followed the performance of this operation amongst us forbade interference, even when necessary, under the most favourable circumstances, far more in those dangerous cases of large, quickly growing, soft fibroids or fibro-cysts. Though the greater number of uterine tumours—if not malignant—give little trouble, rarely interfering with life, and often not even with the comfort of the patient, yet in a certain number they cause the subjects of them to lead useless, dependent, miserable lives, full of suffering, ending only with life. I hope the time is not far distant when many of these unfortunates will look to surgery for relief with as much confidence as those afflicted with ovarian disease now do.

Edinburgh.

HISTORY OF A CASE OF PYÆMIA IN PRIVATE PRACTICE,

IN WHICH RECOVERY TOOK PLACE AFTER EIGHTEEN ABSCESSES HAD OCCURRED.

BY FREDERICK H. DALY, M.D.

(*Concluded from page 471.*)

JUNE 10th to 17th.—The patient's feet became more œdematous and intolerably painful. I tried resting them on an inclined plane, but he said it made them much worse; so I got him one of Salter's swings, with the cradle, and this he found most comfortable. I removed the drainage-tubes from the thighs, as there was scarcely any discharge from these wounds. On the evening of the 18th his temperature was 102°; pulse 150. Next evening the temperature was 104°; pulse 160; and there was very little discharge from any wound, no sign of a fresh abscess, and nothing to account for the rapid pulse; had no appetite. At 7 A.M. on the 14th he had a prolonged rigor, and there was a deep erysipelatous blush all over the right hip and front of the thigh. His temperature was 105·8°. I also discovered a new abscess behind the left trochanter, symmetrical with the one behind the right trochanter. I opened it, and it contained four ounces of unhealthy sanguineous pus. His pulse was feeble, and he looked very badly, almost yellow. The urine was bright-coloured and scanty; no albumen. He had no appetite. There was no discharge from any of the old abscesses.

On the 14th Mr. Hutchinson saw him with me late in the afternoon. The erysipelas had spread all over the back, and to the left side and thigh. Mr. Hutchinson and Mr. Waren Tay examined his blood microscopically, and there was a relatively large number of white corpuscles. In consultation, we agreed to stop the mercury, which he had taken for exactly a month without its producing any effect upon the gums or breath, and again to give him twenty grains of quinine daily with the half-ounce of tincture of steel, and to press fluid nourishment and stimulants. Strong lead-and-spirit lotion to be continually applied to the erysipelatous parts. The erysipelas lasted until the 21st of June, and during this period it continued travelling about from part to part. His prostration was extreme. He became quite deaf, and ultimately became so exhausted that he could scarcely bear the application of the lotion. The nervous prostration and muscular exhaustion were very great, his hands and tongue were tremulous, and the hyperæsthesia was exquisite. Mr. Hutchinson saw him with me during this period, and we despaired of his life, but the free use of stimulants kept him alive. During each twenty-four hours he took twenty-four ounces of port wine, a pint of champagne, and eight ounces of brandy, together with milk, beef-tea, and beaten up-eggs. Some days he was almost in a state of collapse. His urine kept free from albumen, and his temperature was never below 103°, or his pulse below 140.

On the 20th the erysipelas was quite gone, and the same day the left thigh discharged about a drachm of pus, all discharge from the abscesses having ceased during the attack of erysipelas.

On the 21st, his morning temperature was down to 101°; pulse 132. Mr. Hutchinson, when he saw him with me in the afternoon, expressed the most unfavourable opinion of the case. We found him deaf, sleepy, and dull, dozing to sleep while you were speaking to him; no albumen in urine, sp. gr. 1022; feet and legs greatly swollen; his voice was altered, and he looked almost cadaverous; heart and lung sounds normal. Mr. Hutchinson detected fluid in the right knee-joint, and the knee-cap *grated*, but he did not think the fluid was pus, probably only discoloured flaky synovial fluid. The right knee measured three-quarters of an inch more than the left. We increased his port wine to three pints during twenty-four hours, in addition to the brandy and champagne. The extra amount of stimulant agreed with him. Next day he had rallied a little; he was not so sleepy or deaf, and his tongue was cleaner. He took his

fluid nourishment, such as eggs and beef-tea, somewhat better. No discharge from any wound. His voice was still weak, and he could not bear anyone talking in the room.

From the 23rd to the 30th he continued to improve. His appetite gradually returned, and as it did I reduced his stimulants. I also got him to take half a pint of Guinness's stout with his dinner, instead of Swiss wine. His temperature varied from 100·6° in the morning to 104° in the evening, his pulse keeping between 130 and 140. The thighs and shoulders discharged a little thin watery fluid, occasionally smelling unpleasantly. The wounds were dressed with carbolic oil. The swelling of the right knee decreased, but the patella still grated. At the end of the week he was taking in twenty-four hours ten ounces of port and six ounces of brandy, and continued the half-ounce of tincture of perchloride of iron, and twenty grains of quinine. No champagne.

July 1st to 8th.—Improved very much this week. *Copious* healthy discharge returned, for the first three days of this week, to the right hip and thigh, and left hip, thigh, and popliteal space. He says he feels relieved by the discharge. His feet are not so œdematous, or nearly so painful. Removed the Salter's swing. Is in good spirits, eats well, and sleeps well. He has a small bedsore, the size of a sixpence, caused by the bed-pan; to be dressed with carbolic oil, and the back well sponged with whisky. During the latter part of this week the discharge gradually ceased from the five wounds, but they did not heal. Had diarrhœa for a day.

July 8th to 15th.—Was for the first time lifted off the water-bed on to a sofa, an air-cushion being placed under the sacrum, to prevent pressure on the little bedsore, which looks quite healthy and is healing somewhat. His temperature this week varied from 100° morning to 101·6° evening, and his pulse from 124 to 136. The five wounds discharge a very little, and show no disposition to close; feet much less painful and less œdematous; patella does not grate, and the right knee measures almost the same as the left; urine normal; appetite good; continues his steel and quinine; he takes much less stimulants.

At this date I left town for a fortnight, and during my absence my partner, Dr. Long, took charge of the case. From his notes I learn that during this fortnight the improvement steadily continued, the morning temperature falling to 99·4° and the pulse to 120, evening temperature rising to 101·8°, pulse to 136; the discharge gradually ceasing from the five open wounds; bedsore improving; urine normal; no motion in the knee-joints; feet and legs not so œdematous. Mr. Hutchinson saw him with Dr. Long, and advised the quinine to be reduced to twelve grains daily instead of twenty, to continue the steel, to have the legs, feet, and joints well rubbed twice daily with iodide-of-lead ointment, and to again elevate the feet and legs on an inclined plane. Both knees measure alike, and passive motion of the knees and ankles is to be used twice a day.

Aug. 1st.—I again took charge of my patient. He is much improved in appearance. Morning temperature normal, pulse 110; evening temperature 99·8°, pulse 120. Bedsore nearly healed. There is very little motion in the knee-joints, but the feet and legs are much less œdematous. He is lifted each day into a chair, but has no power of standing. There is no discharge from any abscess, and the wounds have cicatrised.

On the 13th he was carried into the garden and lay on a sofa there for a few hours, and the following day was lifted into an open carriage and had a drive. He continued to take a drive daily, when the weather permitted, until he went to the seaside.

On the 28th a small superficial abscess formed on the right instep. From the beginning of his illness there had been a small purple spot where this abscess formed, which Mr. Hutchinson and I had often remarked. I opened it, and it contained exactly three drachms of healthy pus. There had been no rigor or rise of temperature.

There being little improvement in the motion of his joints, I requested Mr. Hutchinson to see him again with me, which he did on the 3rd of September, and with the view of forcibly flexing the knees, I gave chloroform to complete anæsthesia, when Mr. Hutchinson found that there were no adhesions in the joints, but that the rigidity was due to inability on the part of the muscles to relax. The flexors and extensors were equally affected; only a very limited range of movement was permitted, and then the sense of fixation was as if the muscles were *absolutely non-extensile*, and must be ruptured if force was used. We did not therefore use very forcible flexion, bending the knees only to an obtuse angle. More force would have either ruptured the muscular fibres or fractured the patella. We agreed to apply a galvanic current to the legs twice a day, and he was to use them as much as possible, by sitting on a high chair and swinging them. The nurse was also to bend them well for a quarter of an hour two or three times a day, and he was to try to walk, supported on either side by one person. To discontinue the quinine which he had taken for more than five months (twenty grains daily). He was still to take fifteen drops of tincture of perchloride of iron three times a day.

He continued to improve, and on the 19th September I sent him to Hastings with his nurse, where he remained for three months. I made him take with him a pair of crutches, and he was soon able to walk with them. During his stay at Hastings he had two attacks of erysipelas, but, with the exception of these drawbacks, the improvement was continuous. I visited him at Hastings on the 7th of October, and he could then walk well from room to room with crutches. On his return to London his walking powers still further improved, and by the 11th March, 1874, he was sufficiently well to go to Liverpool, to resume the office work of his business there, and could walk three or four miles. Before leaving town he got Messrs. Walters, of Moorgate-street, to fit him with a pair of elastic stockings, and he felt them a great support.

On the 7th of May he wrote to me from Liverpool—"I am improved a great deal in every respect, especially in my walking. My feet are still a little swollen, but decidedly less so than when last I wrote. In all other respects I feel *perfectly well*;" and he continues so.

Queen's-road, Dalston.

A Mirror OF HOSPITAL PRACTICE,

BRITISH AND FOREIGN.

Nulla autem est alia pro certo noscendi via, nisi quamplurimas et morborum et dissectionum historias, tum aliorum, tum proprias collectas habere, et inter se comparare.—MORGAGNI *De Sed. et Caus. Morb.*, lib. iv. Proœmium.

ST. THOMAS'S HOSPITAL.

GASTROSTOMY FOR STRICTURE (CANCEROUS?) OF ŒSOPHAGUS; DEATH FROM BRONCHITIS FORTY DAYS AFTER OPERATION.

(Under the care of Mr. SYDNEY JONES.)

FOR the following notes we are indebted to Mr. S. Osborne, registrar.

Charles L——, aged sixty-seven, engineer, was admitted on the 21st of January. He had had difficulty of swallowing for ten months, and for seven months had been unable to swallow solids. He had lost his voice for four months, although there was no difficulty of breathing. A bougie did not pass beyond the commencement of the œsophagus; and on bending the head forwards, so as to relax the front cervical muscles, a hard tumour could be felt on the left of the trachea, behind the sternal end of the clavicle, too low down to promise anything from attempt at œsophagotomy. He had lost very much flesh. Previously stout, his abdomen was now fallen away, the ensiform cartilage projecting as a hook-like process. He complained much of hunger, and of persistent sinking pain at the pit of the stomach. His difficulty of swallowing liquids was increasing; and a constant discharge of mucus from his pharynx caused him much trouble. The pulse was 96, feeble, occasionally intermitting. The urine was 1014, non-albuminous. The man himself was anxious to have some interference. He was intelligent, and cheerfully expressed his willingness and desire to have some operation done which would relieve him of his discomfort and sensation of hunger. He did not complain of pain in the neck tumour, which was discovered only after exploration for the cause of the stricture. It

was impossible to do an œsophagotomy. The risks of a gastrostomy were fully detailed to him; and it was also explained that such operation, if successful, could be the means only of alleviating, not of curing his symptoms. He was anxious to have recourse to any means which would afford him relief.

March 3rd.—2 P.M.: Chloroform having been administered, gastrostomy was performed by Mr. Sydney Jones. It had been ascertained by previous experiment that the readiest mode to secure the stomach, and in the best position, was to incise along a line drawn from the outer border of the left nipple to the outer border of the spine of the pubis on the same side. An incision was made along this line for about three inches and a half, beginning about an inch below the costal cartilages. The outer border of the rectus muscle was easily hit; no muscular fibres were divided; only a little venous hæmorrhage occurred; and the stomach was easily caught and at once drawn through the opening by the finger and thumb passed into the peritoneal cavity. The stomach was brought into connexion with the side of the wound by sutures, the rectus muscle being included in the sutures, and the edges of incision above and below brought together by needles. Temperature 98·6° F. previous to operation; after operation 97·7°. Before the patient was removed from the operating table an enema of milk and brandy with half a drachm of tincture of opium was administered. About 4 P.M. a subcutaneous injection of morphia was given on account of some restlessness, which continued up to 6 P.M. Temperature at 10 P.M. 99·6°. The following day the patient was troubled with some spasm of abdominal muscles, which occurred every ten minutes, and went on until the evening. The pulse was also found to intermit at times. Enemata were administered every four hours.

5th.—Patient smoked a pipe of the strongest shag tobacco this morning. Had no pain around the wound. Spasms still continued at intervals.

6th.—Was in good spirits without a bad symptom. No spasms had occurred since 10 P.M. the previous evening. He was troubled with flatulence. Had three pipes during the day. The needles were removed from the wound, around which there was a slight redness. A discharge of a clear fluid, neutral in reaction, escaped from the stomach on coughing.

8th.—Fed for the first time by the stomach; had one ounce of milk with brandy. Redness about the wound not increased; sutures were ulcerating through. On account of complaining of pain from lying on his back, he was allowed to lie on his right side.

9th.—Patient was very cheerful, and was allowed to take nourishment by the mouth. He was going on well without a bad symptom; his temperature remained normal; a large amount of greenish secretion came from the wound; no tenderness of abdomen. The lower edges of the wound were callous from the constant passage over them of the secretion from the stomach. The patient was still troubled with slight flatulence; again fed by fistulous opening.

10th.—During the day the nourishment he took consisted of six ounces of milk by the mouth; six enemata, each consisting of nine ounces of milk, one ounce of brandy, and one egg. He also had seven pipes. Two of the sutures came away on the 14th, the other on the following day. The flatulence still came on after taking any nourishment.

23rd.—Fed twice daily by the stomach. The enemata were still continued, and the patient was allowed to take small quantities of milk, ice, and jelly by the mouth. On the 26th he was up and sitting by the fire, being anxious to be allowed to go out.

April 4th.—Somewhat depressed; spat up a small quantity of blood; mucous expectoration still continues from his throat, but not more in quantity than before the operation.

7th.—Had slight bleeding from the throat, and two dark clots were found at the wound. Otherwise the patient was in good condition.

9th.—Again had some bleeding from the mouth mixed with the expectoration, but his spirits were good.

12th.—Appeared very depressed; no more bleeding from throat. Expectoration stopped suddenly last night, and symptoms of accumulated bronchial secretion became more and more urgent, and terminated fatally at 2.15 P.M., forty days after operation.

At the post-mortem examination there was found a broken-down cancerous growth invading the pharynx, springing from just below the upper border of the cricoid cartilage, and extending for three inches and a half downwards, and also infiltrating the left pyramid of the thyroid body. The new growth overlapped the mucous membrane, which was reflected underneath the overhanging edge, rendering it probable that the growth sprang from the areolar tissue of the pharynx, or the parts outside it. There was atrophy of the left optic nerve and tract from old injury and loss of eyeball; also loss of substance of the under part of the left anterior cerebral lobe, with orange-coloured discolouration from old blood extravasation after injury; a similar change had taken place in both olfactory lobes. There was also great excess of mucus in the lungs, the result of bronchitis—the actual cause of death. Union was complete and firm between the stomach and abdominal wall.

SEAMEN'S HOSPITAL, GREENWICH.

CASE OF PSEUDO-MUSCULAR HYPERTROPHIC PARALYSIS.

(Under the care of Dr. RALPH.)

IN a leading article of THE LANCET of April 3rd we drew attention to the disease known as "pseudo-muscular hypertrophic paralysis," which was first recognised and described by Duchenne. We now publish the notes of a case at present under treatment at the Seamen's Hospital, and which presents some features of special interest. We have to thank Dr. Law, the resident physician, for the description of the history and present condition of the patient.

M. C——, aged twenty-one, a sailor, was admitted on the 3rd March, 1875, complaining of pain, weakness, and swelling of the thighs. He states that he has been a sailor for four years; cruising backwards and forwards in a coasting vessel to Scotland up to the time of his admission to the hospital. Latterly, however, he got through his work with difficulty, and during his last voyage had to keep his berth on account of weakness and the pain in the affected limbs. His general health has always been good; but, as long as he can remember, he has never been very strong on his legs, and from childhood has occasionally experienced darting pains in both lower extremities, more particularly the right, occurring usually about the middle of the day and after exercise. About four years ago he noticed swelling of the legs below the knee, which symptom persisted about three years; at the end of which time—viz., about a year ago—he was admitted into the Hull Infirmary, where he was treated for rheumatism, and remained nearly three months. Since leaving Hull Infirmary his thighs have increased in size, while the calves of the legs have diminished. He has never suffered from syphilis, and is of temperate habits. His father is alive, and suffers from chronic rheumatism, but is otherwise healthy; mother died during his infancy, he believes from rheumatism and dropsy. Has five brothers and one sister living; the latter is crippled with chronic rheumatism. Two brothers and one sister have died, but he does not know from what causes.

The patient walks in a slow and somewhat laborious manner, slightly stooping forward, and swinging the right leg, which he does not bend, as it gives him pain in the middle of thigh. When stripped both thighs are seen to be enlarged, and the muscles at the upper, anterior, and outer parts are prominent and bulging, and give the appearance of considerable muscularity. The calves are relatively much too small for the thighs. The glutei, abdominal, and spinal muscles, and those of the upper extremities, appear natural in outline. The thighs are tender to the touch, and especially sensitive to firm pressure over the middle and upper anterior portion. The skin of both thighs has a mottled appearance, faintly marked, irregular congested patches being observable. The measurements are as follows: height, 5 ft. 8 in.; weight, 162 lb.; chest, 36 in.; thighs, both alike, upper part over trochanters, 23¼ in.; middle portion, seven inches below crista ilii, 22¼ in.; eleven inches below crista ilii, 20 in.; just above patella, 15¾ in.; calves, thickest portion, 12 in. There is considerable loss of muscular power, and the natural movements of the legs can be produced but slowly and laboriously. Common sensation appears normal. Temperature of thighs equal, 98·5°, of calves 97·4°; electro-motility equal in thighs and legs;

electro-sensibility greatest in thighs; thermo-sensibility greater in thighs. Complains of more or less constant aching in thighs and loins, increased by exertion; lancinating pains in thighs shooting upwards; the pains are worse during the day than at night. Fibrillary twitching of the muscles of the thighs easily excited. Optic discs: right, slight shading of entire margin, about one-third of its radius, arteries numerous and large; left, the shading is less noticeable than in the right, arteries similar. His appearance is dull and heavy, but his intellect is not impaired. Visceral organs healthy. During his stay in hospital he has had a slight epileptic seizure. Treatment: one ounce of iron mixture three times a day, belladonna liniment to be rubbed into the thighs, and full diet.

Dr. Ralfe, in some clinical remarks, observed that hypertrophic muscular paralysis was usually met with in young children or in quite young adults. Barth (*Archiv der Heilkunder*, xii., 1871,) indeed mentions the case of a man aged forty-four, who died of this disease, but such a case is exceptional. In the present instance muscular weakness and pain were complained of during childhood, but it was only when additional strain was put on the muscles, when the patient undertook the laborious duties of a sailor's life, that they became hypertrophic. The patient states the swelling first began in the legs below the knee, and this swelling of the legs continued for three years, when his thighs began to enlarge and the calves to diminish. If this statement is correct, the fact is interesting, for Dr. Ralfe believes there is only one other case recorded in which muscles, previously enlarged, subsequently became atrophied. A difference of 1·1° F. (taken as an average) is noticed between the temperatures of the enlarged thighs and the calves; in Dr. Ord's case (*Med. Chir. Trans.*, 1874) the excess of temperature in the enlarged calf over normal thigh was 1·8° F. to 3·9° F. This excess of temperature, together with the irregular patches of vascular congestion noticeable over the affected limbs, Dr. Ord has regarded as indicating that the disease is in some way connected with vaso-motor disturbance. Nothing at present is known respecting the etiology of this disease. In this case both parents suffered from rheumatism, and one sister is crippled with it. If a family history of rheumatism could be obtained in these cases, Dr. Ralfe thinks we might regard the excess of intermuscular fibrous tissue which marks the early stage of the disease as caused by the rheumatic tendency, the subsequent disappearance of the muscular fibres and increase of adipose tissue being a degenerative change produced by long-continued faulty nutrition. Any way, the family history of those cases ought to be minutely inquired into.

BIRMINGHAM GENERAL HOSPITAL.

SUCCESSIVE ANEURISMS OF THE LEFT AND RIGHT POPLITEAL ARTERIES CURED BY PRESSURE AND FLEXION.

(Under the care of Mr. GOODALL.)

WE are indebted to Dr. Walter Ottley for the following notes.

The patient, a man aged thirty, spare, and of dark complexion and unhealthy appearance, brassfounder, was admitted with aneurism of left popliteal artery on July 10th, 1874. His history was as follows:—About four months previously he fell with his left leg bent under him. This was the only ascertainable injury. Five years ago he had rheumatism, but there was no history of syphilis. Nine weeks before admission he noticed a fulness in the left popliteal space. This swelling steadily enlarged, but did not prevent him working until the previous week. His work involved a good deal of lifting, standing about, and other muscular exertions.

On admission, the whole popliteal space was occupied by a smooth, oval, expansile swelling. There was no redness of skin, but the leg and foot were œdematous, and the superficial veins enlarged. There was considerable pain in the vicinity of the tumour, and down the leg. The circumference of the left knee was 15 in., that of the right 12¼ in. There was no cardiac murmur, nor any evidence of general atheroma.

July 23rd.—Treatment was commenced by flexing the leg to an angle of 50°, which had the effect of completely arresting pulsation. Hypodermic injections of morphia were given rather frequently in consequence of the great pain generally complained of after tightening the strap from the heel by which flexion was maintained. Slight pulsation returned in a few hours, requiring a slight increase of the flexion.

28th.—Some enlargement of superficial vessels was noticeable, but the aneurism continued to pulsate till Aug. 11th, when a shot bag was applied to the femoral artery, and kept in position by the patient, flexion being still maintained. On the following day, Aug. 12th, at 9 P.M., on removing pressure, no pulsation was discernible, nor did it reappear, though there was fluctuation in the tumour for many days after this date. Duration of treatment, twenty days.

He was discharged cured on Sept. 16th, with the knee still slightly bent.

On Jan. 7th, 1875, the patient returned to the hospital with an aneurism of the right popliteal artery. He first felt pain across the front of the right knee, similar to that which he had previously experienced in the left knee. On examining his popliteal space he found a small pulsating tumour, about the size of a nut. On Christmas day he had fallen and strained this leg, which he assigns as the probable cause.

Jan. 8th. — A hard, smooth, movable, non-pulsatile tumour, the size of a pigeon's egg, could be detected in the seat of the former aneurism—viz., the left ham, extending under the gastrocnemius muscle. In the right popliteal space a globular swelling, tender to the touch, about two inches by three inches, could be felt occupying the middle of the space, and pulsating freely, with a distinct thrill over its centre.

22nd.—Treatment was commenced by suspending (from a cradle) a shot-bag over the femoral artery in the groin. By this means pulsation was readily stopped.

26th.—The tumour was perceptibly smaller and harder. The patient kept the bag in place during the greater part of the day, taking it off at meal times and for a few hours at night, but its removal always increased the pain.

29th.—Weight increased from 5½ lb. to 6½ lb.

Feb. 13th.—Flexion was added to the shot-bag, as the patient had not kept up pressure satisfactorily for a day or two.

17th.—The tumour was more consolidated, but there was still some pulsation.

23rd.—The digital compression was instituted with flexion for ten hours, by which time pulsation had ceased. It returned, however, slightly on the outer side, and the shot-bag was again applied.

March 10th.—It was intended to try digital compression again, but on the previous night pulsation had ceased.

11th.—All pressure was discontinued. The aneurism was harder, but not yet completely consolidated.

23rd.—Patient convalescent; was allowed to get up for a few hours. Duration of treatment, forty-seven days.

Medical Societies.

ROYAL MEDICAL AND CHIRURGICAL SOCIETY.

THE ordinary meeting of this Society was held on the 11th of May, Sir James Paget, President, in the chair. Three papers were read on subjects of considerable interest, but the time did not permit of any extended discussion. Dr. Galabin showed a new form of cardiograph, which consists in such a modification of Marey's sphygmograph as to allow of its application to the heart's apex.

The first paper read was "On the Histology of the so-called Nutmeg Liver," by Dr. J. WICKHAM LEGG. The author, after reviewing the opinions which have been held concerning the anatomy of nutmeg liver, gave the result of his examination of twenty such livers. He found the vessels in the centre of the lobules dilated, with a corresponding atrophy of the liver-cells around them, but with

no new growth of connective tissue in the centre of the lobules, as Rokitansky, Frerichs, and Virchow have held. He found, on the contrary, that the growth of the connective tissue is well marked in the capsule of Glisson, and between the lobules, just as in the early stage of primary cirrhosis, and he looks upon this overgrowth as the cause of the shrinking and hardening of the liver in advanced stages of nutmeg liver. Other cases of the overgrowth of the connective tissue in organs subject to persistent hyperæmia were brought forward; and the paper ended with a protest against the name of red atrophy given to the atrophous nutmeg liver by Rindfleisch.—Dr. THIN inquired whether Dr. Legg thought that connective tissue passed between the rows of liver-cells in a lobule; he had only referred to blood-vessels, and had made no mention of connective tissue which the mode of preparation employed might have altered.—Dr. GREEN said that the author had shown there were two distinct kinds of change in a nutmeg liver—namely, first, atrophy and pigmentation of liver-cells in the centre of each acinus; and, secondly, an increase of connective tissue at the margins. The first is the most important part of the process, and he thought too much stress had been laid upon the second element. He had not seen such marked connective-tissue overgrowth, especially nuclear, in the portal canals in these cases. The indurative changes set up in organs by chronic congestion were due to increase in connective tissue, often nucleated, and not to be distinguished from the appearances due to chronic inflammation. As a rule, it differs in being less nucleated, and it is certainly a question as to how much of the connective tissue in these cases is due to new growth, and how much to atrophy of vessels, &c. At any rate it is certain that some amount of new growth results from chronic congestion.—Dr. HANDFIELD JONES said that when working at the subject he had concluded that chronic inflammation was insufficient to account for the changes described. He had examined the liver in all stages of cirrhosis, and had never fairly satisfied himself that the process was an inflammatory one. What then gave rise to the growth of connective tissue? It was often ascribed to spirit-drinking, but this was insufficient, alcoholism more often producing fatty degeneration. He had thought rather this fibroid degeneration of organs was part of a general change occurring in different parts of the body at the same time; a change opposite to that met with in phthisis, where the new-formed tissue goes on to a cheesy condition. Nor had he ever during life been able to ascertain a previous stage of inflammation in cirrhosis of the liver.—Dr. LEGG, in his reply, stated that he was fully aware of the long-standing controversy as to the existence of sheaths of connective tissue around the columns of liver-cells, in a lobule, proceeding from the portal canals to the hepatic vein. He thought "chronic inflammation" an ill-defined term, and would be the last to attribute the changes he had described to any such cause. He had speculated much as to the causes of cirrhosis, but had never been able to formulate theories worthy of acceptance.

A paper by Mr. HENRY LEE was then read, entitled "Sequel to a Paper on Excision of the Ankle-joint." In a former communication of Mr. Lee's, on Resection of the Ankle-joint, the plan of dislocating the lower extremity of the tibia and the upper surface of the astragalus outwards was advocated as the best mode of performing the operation, but it was demonstrated that before this could be done the internal malleolus must be removed. A division of the internal lateral ligament is not sufficient; the projection of the internal malleolus when present prevents the foot being turned sufficiently inward as long as the upper surface of the astragalus is held in apposition with the lower extremity of the tibia by the tendons which pass behind the ankle-joint. The difficulties of dislocating the bones in order to perform the operation satisfactorily were considered in all the plans which had previously been suggested. A case was given, which was believed to be the only one on record, where a complete primary resection of the ankle-joint was performed, and in which a most satisfactory result was obtained. In this case the bones had been dislocated outward, and the extremities of the tibia and fibula were removed without any attempt at reduction being made. On the 27th of January last another case fell under Mr. Lee's care, in which a similar compound dislocation of the ankle had occurred; but in this case the lower extremity of the tibia was dislocated inwards, and projected for two or three inches through the skin. The extremity of the internal malleolus had been detached, and remained connected with the internal lateral ligament. The fibula was broken about three inches from its termination. In this case the lower extremity of the tibia was sawn off, and the bone replaced in its normal position; an incision was then made over the external malleolus, and the periosteum separated from the bone as much as practicable. The external lateral ligaments were then divided, and the lower broken portion of the fibula removed. The limb was then again placed on its outer side, and the upper surface of the astragalus made to protrude as much as possible through the internal wound. A deep horizontal groove was made by means of a small saw below its upper articular surface. This surface was then removed completely by means of the cutting-pliers; the two smooth surfaces of bone were then placed in apposition, and carbolic dressings applied to the wound. A very severe attack of erysipelas occurred on the fourth day, which extended up the leg and thigh, the skin being of a very deep livid colour. It was feared that gangrene of the limb might take place, as had occurred in another patient who had the same symptoms, and who was admitted about the same time with a compound fracture of the leg. A deep incision was made on each side of the leg, from which there was soon a copious discharge of pus. The skin now assumed a bright-red colour, and the erysipelas, having run its course, gradually subsided. The portion of the astragalus which had been removed had, and still has, attached to it a portion of the anterior tibial artery, which had been torn through at the time of the accident. The case previously recorded offers some interesting points of contrast with the present one. In the first the bones were dislocated outward, in the second inward. In the first the internal malleolus was not removed, although detached from the tibia, and the fibula in that case was not broken. The internal malleolus, which was allowed to remain, proved to be the cause of some subsequent irritation and suppuration, and the conclusion was arrived at that both malleoli ought in this operation to be removed, whether detached or not. The principal point, however, to which the author wished to direct attention, was the much greater facility which there is in removing the upper surface of the astragalus where that bone is dislocated outward, than when an attempt is made to displace it inward. This, in the author's opinion, makes a very great difference where a surgeon can choose which operation he will perform. Even after both malleoli are removed, the powerful tendons which run behind the joint on its inner side tend to prevent the astragalus from being inverted, and are of themselves sufficient to account for the difficulty that surgeons have experienced in attempting to turn the astragalus sufficiently inward to remove its upper articulating surface. This he did without difficulty in the first case, by a clean section through the entire upper part of the bone, but he could not do so in the second without distressing the surrounding parts more than might be desirable. After the saw had been used to a certain extent, the upper surface of the astragalus had to be removed in three different portions by the cutting forceps. — Mr. MAC CORMAC had brought to the Society a boy ten years of age in whom he had performed complete excision of the ankle-joint for injury. In the operation, which he believed had long been followed by Langenbeck, the malleoli and surfaces of tibia and astragalus were removed. The mode of operation differs from that pursued by Mr. Lee in the first incision being made on the outer side, vertically down the fibula. The periosteum is dissected off from the bone which is removed. The upper surface of the astragalus is removed by a small saw before any incision is made on the inner side, and thus more support is given to the bone while it is being sawn through. Then an incision is made along the inner side of the joint, and the articular surface of the tibia is removed. The boy was able to walk two months after the operation; recovery was very good. Mr. Lee's was not the first case of primary excision on record; it had been performed for gunshot wound in the Franco-German war.—Mr. LEE had purposely omitted any complicated cases, and confined himself to simple excision, of which he thought his cases were the first that had been described. It was easier to begin the operation by an inner incision, and to remove the internal malleolus, than to follow the method

pursued by Mr. Mac Cormac. But where there is much thickening from chronic disease or inflammation the removal of either malleolus is difficult. It is then best to dislocate outwards. In both his cases there had been complete restoration of the malleoli. He thought when the fibula was broken high up it would be advisable to remove the whole fragment.

A paper followed "On the Construction and Use of a New Form of Cardiograph," by A. L. GALABIN, M.D. The author pointed out the liability to error which exists in Marey's cardiograph, and in the modification of it by Dr. Burdon-Sanderson, in consequence of the use of a flexible tube containing air to transmit the motion. Two effects may be produced in this way: first, the rounding off of any abrupt features in the curve; and, secondly, the introduction of oscillations. It has been shown by Dr. Rutherford that, if even a slight impulse be communicated to the instrument by the finger, oscillations do occur. The mode of verification adopted by Marey, in which a motion already known was communicated to the instrument by the rotation of the eccentric, proves only that such a curve as that described by his cardiograph can be transmitted a second time without further considerable change, and not that the original cardiac motion could be accurately depicted. The most perfect cardiograms hitherto obtained have been those procured by the application of the sphygmograph directly to the apex of the heart. The sphygmograph cannot, however, be generally used in this way in cases of heart disease for two reasons:—In the first place, the extent of the cardiac motion varies very widely, and when the heart is hypertrophied it becomes far too great to be recorded; and, in the second place, when the sphygmograph is applied to the chest, not only the spring-pad, but the framework is influenced by the cardiac impulse in cases in which the apex-beat is diffused, and in this way the curve described becomes modified. The cardiograph invented by the author is such a modification of Marey's sphygmograph that these two difficulties may be avoided, and tracings obtained in all cases direct from the apex of the heart. In order to attain this object, the knife-edge by which motion is communicated to the recording lever is not rigidly connected to the spring pad, but is attached by a sliding bar, which can be fixed in any position by a screw. In this way the amount of amplification given to the motion can be varied from about ten to a hundred times the original. The instrument is also attached to a movable frame in such a way that the wooden bars on which it rests can be separated to a width of five inches. And the brass-work can be raised or lowered at either end. It can thus be adapted to a chest of any size or shape. In tracings obtained by this cardiograph, the thrill which accompanies a murmur is often depicted as a vibratile line in the curve, and then the exact relation of the murmur to systole and diastole is permanently recorded. The shape of the systolic part of the curve indicates the proportion which hypertrophy bears to dilatation, while aortic regurgitation is shown by a rapid ascent during diastole. In many cases of mitral contraction an increase and prolongation of the auricular elevation is seen, while in some others, in which a direct mitral murmur appears to be due to the venous flow, the auricular systole is either not discernible or it appears in its usual position and not prolonged. The cardiograph shown was made by Mr. Clark, of 3, Windsor Cottages, High-street, Lower Norwood. Instruments of the kind may be obtained through Messrs. Krohne and Sesemann, Hawksley, or Weiss.

The meeting then adjourned.

PATHOLOGICAL SOCIETY OF LONDON.

THE following is the reply of Dr. Bastian on May 4th, at the conclusion of the debate on the Germ Theory of Disease.

Dr. BASTIAN.—I think the Society may congratulate itself that the debate did not end prematurely at the last meeting. In the first place, because we have had an opportunity of hearing the very able speech of Dr. Murchison, and also because many additional observations have been made by members of the Society. I think it will facilitate matters if I first of all refer to questions touched upon in the debate this evening. With regard to Dr. Murchison's speech, I have no particular comments to make, because his remarks almost entirely accord with those which I should be inclined to offer myself, only that on many subjects of course he possesses a knowledge that I do not possess, therefore I am only too pleased to have heard his views on the subject. With reference to Mr. Wagstaffe's observations on blood in various diseases, I am anxious to make a few remarks. In the first place, I may say in the years 1869 and 1870, I did submit a very large number of specimens of blood in various diseases, mostly acute diseases, to long and careful observation, and I detailed notes taken of each examination; I may say on each examination I expended sometimes an hour or an hour and a half, and I made very careful notes and drawings afterwards. In all these examinations made in cases of typhus fever, erysipelas, and many other affections, I never once saw a single object which I could put down as a bacterium, or which I, in my own experiments, would have ventured to cite as a bacterium. I saw, of course, many times a number of granules in the blood, more especially in cases of fever, where granular matter is extremely abundant, and in many cases the granules would possess a much more active motion than the granules of the blood usually do. I do not pretend to understand what is the cause of that active motion of these granules, but I would not venture to say that these were independent organisms at all, and I have never yet seen in the blood of any living patient independent organisms. I have examined the blood from a pyæmic patient, patients dying of pyæmia, with the greatest care, over and over again. Last year I again renewed the examinations, and never was able to find a trace of anything like a bacterium in the blood of a living person suffering from pyæmia. A very great number of observations have been put upon record with regard to the presence in the blood of these micrococci. I have a distinct objection to that word "micrococci," because it means a mere granule, a thing with no specific or definite form at all, and I have the same objection to the word "microzyme," because that has been used in precisely the same sense, and means nothing. I would never venture to say in any given case that the blood contained organisms unless I saw that it did contain organisms having a definite shape. We get granules everywhere. Let anyone boil a little albuminous urine and look at the granules there. You get it swarming with micrococci. You may say that these are organisms. I know of no means of distinguishing between these granules of precipitated albumen and the mere granules which so often pass as micrococci; and therefore, unless the things do possess a definite form, I should be very reluctant to say that they were organisms at all. Then Mr. Wagstaffe mentions acute syphilis. I have many observations recorded of syphilis, acute and chronic, and have never been able to trace anything in the blood in that disease which had the least appearance of bacteria. So far my observations seem entirely to accord with those which have been made on this subject; and they accord with those of Professor Stricker, Professor Billroth, and many other observers, even as regards pyæmia, for these observers have failed to find bacteria in the blood of living patients suffering from pyæmia; and the same accord also with the observations made by Dr. Goodhart and Dr. Moxon. Dr. Goodhart mentioned that I brought some observations to the notice of the Society in 1869 in which bacteria were found present in the blood; but I really do not recollect that myself. I know I have mentioned elsewhere the fact that bacteria are found abundantly in the blood in cases of persons dying with a very high temperature—say 109° or 110°; that bacteria then occur in all the vessels in different parts of the body in the greatest abundance. And this leads me to remark upon a certain statement made by Dr. Payne. Dr. Payne says that he placed no significance upon the occurrence of bacteria in the alimentary canal. He says, truly enough, we are hollow animals, and bacteria may well get in from without. But it is not only in the alimentary canal that these rod-like or ordinary septic bacteria are met with; they are found just as abundantly within the epithelium in the ducts of the skin, and in other places, and they are invariably found in all parts of the body

after death. It is only a question of time, though the actual time again is a question of temperature. If the temperature is high, organisms are formed very rapidly indeed, within a few hours even, in the different organs of the body. If the temperature is low, organisms are formed more slowly. There is again, it is true, another disturbing condition, and that is the state of health of the patient. If the patient dies in full health, the fluids and tissues of the body may be less prone to undergo this change, and early bacteria are not found anything like so frequently; but if a patient dies who has been half starved and subjected to a very high febrile temperature, or in whom gangrene has occurred in a certain portion of the body, and whose blood may therefore have been poisoned, then the fluids of that person's body are devitalised, as it were—made more akin to organic fluids existing outside the body—and they are proportionately prone to pass over into putrefactive changes, in which, as I maintain, bacteria are bred as a mere natural consequence. I will now pass to the consideration of questions which have been raised in previous debates, and first of all I would make some reference to the remarks of Dr. Sanderson. Dr. Sanderson commenced by reiterating, as it were, certain facts which are not at all denied, and he rather repudiated anything like belief in the germ theory of disease as a theory. Still, I am compelled to say that this theory is his theory practically, because in all his writings, from 1870 onwards to those which have been published even within a few months, he speaks in this manner. He says that the phenomena of the multiplication of contagion within the body are of such a kind that they seem only explicable on the notion of organic multiplication and reproduction. Well, if contagia are increased by organic multiplication and reproduction, it must either be by the organic multiplication of fragments of living matter which have come from a person suffering from the disease, or by the multiplication of self-existent particles—independent organisms—bacteria, in fact. The members of this Society heard the curt way in which Dr. Sanderson dismissed as a matter which could not for a moment be entertained the supposition which has been advanced by Dr. Beale, that contagia are particles which have been thrown off from the living body subject to disease. Therefore, what remains for Dr. Sanderson if the phenomena of contagion could only be explained on that supposition? There is only one conclusion which remains for him—that contagia are independent organisms; and I may say that the tenour of all his writings, from 1870 onwards, has been to promulgate that opinion. In support of this I would refer to the Blue-book recently published by the medical officer of the Privy Council, in which he speaks of Dr. Sanderson's researches—which of course were made under the direction of the medical officer of the Privy Council. This is the view the medical officer takes of these researches. He says, speaking of Dr. Sanderson's observations in 1870: "At that time the general conclusion already seemed justified, first that the characteristic shaped elements which the microscope had shown abounding in various infective products are self-multiplying organic forms not congeneric with the animal body in which they are found, but apparently of the lowest vegetable kind; and, secondly, that such living organisms are probably the essence or an inseparable part of the essence of all the contagia of disease." Then, again: "In the first paper Dr. Sanderson brings down to the present time an account of the Microphytes of Contagion." And further on Mr. Simon says, in reference to the observations of Dr. Klein on sheep-pox, "Dr. Klein has been able to identify the contagium particles of that infectious fever as definite microphytes growing and fructifying with vast rapidity in the canals and tissues of the infected skin." In the Blue-book No. 2 he made as definite a statement concerning typhoid fever; so that we need not go to the letter of Professor Tyndall to *The Times*. Mr. Simon himself expresses the opinion that Dr. Klein has discovered the contagium of typhoid fever in these microphytes which he has recognised in the intestines. Then, again, Mr. Simon speaks of common diarrhœa as being produced by the common septic ferment. Again, he even goes so far as to speak of the origin of phthisis—tuberculosis—in this common septic ferment. He says, speaking of experiments which have been made concerning the artificial production of tuberculosis, that "certain of these experiments have shown that a locally originating cause or contagium appears to be the common septic ferment, or a ferment not yet separable from the septic." Now, I maintain that when we find that the papers of Dr. Sanderson deal with what he calls the facts of coexistence, that he builds up these facts of coexistence, that they appear in blue-books issued by Mr. Simon, and that Mr. Simon takes this most definite position, there seems to be but one conclusion to be drawn. The remarks I have now read may be taken as an answer, if any answer were needed, to the remarks which were made at the last meeting by Dr. Maclagan, when he said that there ran through my opening address the assumption that germ theorists believe that a causal relationship existed between bacteria and the morbid processes with which they were associated, and further added that that was a position which I had created for the germ theorists, and not they for themselves. Now, that did surprise me very much; and after what I have read from this Blue-book, and after what anybody may read in French, German, or American literature, it must surprise anyone who is conversant with the subject. That being the case, it does seem to me a little curious to find Dr. Sanderson anxious to waive all these particular questions which can alone throw any light upon the real relationship existing between the organisms and the morbid processes. But, however surprised I might have been at this, I was still more surprised to hear him state that these questions were wholly beside our pathological mark. If this were really true, it would have been something like a censure upon the Council of this Society and upon me for bringing forward questions which ought not to be brought forward in this Society; but I trust that the Society will feel that I did bring forward questions which were really of pathological interest. The definite position which I set myself to prove was one that I held to be of immense importance—the position that bacteria were not causes of disease, but that they were consequences of disease; that they were, in fact, actual pathological products engendered within the fluids and tissues of the body. I was the more surprised to hear these statements by Dr. Sanderson when I recollected what he had said in 1870. At the close of his paper on the Intimate Pathology of Contagion, he reserved certain important problems for future discussion; and the fourth, I think, of these important problems was worded thus: the question whether microzymes or bacteria "can arise *de novo* in living tissues in mere consequence of impaired activity of nutrition." Well, without pretending to reconcile these discordant statements, I can only say that I prefer Dr. Sanderson's opinion of 1870 to the opinion which he has uttered here in 1875. Then I have been asked by Mr. Jonathan Hutchinson why I attempted to discuss at the same time the question of the relation of organisms to virulent inflammations and to specific fevers. This question has been to a considerable extent answered, and answered very ably, by Dr. Murchison, but there are a few additional remarks which I may also make upon this subject. Mr. Hutchinson believes in the applicability of the germ theory to specific fevers, although he does not believe in its applicability to contagious inflammations. I, however, reject it for both, and I took up the question because it had been asserted for both, and because I considered that a great many considerations which were inimical to the theory in one case were inimical to it in the other, and that, therefore, one might dispose of both, or attempt to dispose of them, at the same time. Mr. Hutchinson, I think, will bear me out in this that, though he does not believe in the applicability of the germ theory to the contagious inflammations, that is a view which is upheld by very many, both in this country and on the Continent. We have heard, for instance, what Mr. Simon says, we know the views of Prof. Lister, and we know the views of many on the Continent, so that the theory is, in fact, brought forward by some pathologists as much in relation to these inflammations as in relation to specific fevers. Still there are, perhaps, many who might be inclined to take the same view as Mr. Hutchinson on this point, and I would like to make this further observation. The whole question comes to depend upon what view you take of the specific nature of these organisms. It is supposed that in the case of contagious inflammations we have to do with non-specific or common ferments, and in the case of specific fevers that we have to do with specific organisms. That is a point which I do not at all recognise, and which most people who have looked at the question from my point of view do not recognise; they do not believe, in

fact, in bacteria as organisms having anything like specific differences in the ordinary sense of the term; they believe, indeed it is admitted by Dr. Sanderson, that bacteria are organisms of the lowest kind, changeable to the very highest degree, capable of being modified by almost every change in their environment. And I may say that, for the most part, the germ theorists do not go into this subject very closely; but those who have, and notably Professor Hallier, and also Professor Lister, are bound to admit that these organisms do exhibit a most protean variability. With regard to the changes which the observations of Professor Lister, made upon bacteria in different media, have revealed, I should like to read the following sentences from his paper on the "Natural History of Bacteria." Speaking of the wonderful changes which take place in bacteria under different circumstances, he says: "Hence any classification of bacteria hitherto made, from that of Ehrenberg to that of Cohn, based upon absolute morphological characters, is entirely untrustworthy. In order to determine the species of any specimen, it is necessary to take into account not merely its appearance, but also the character of the medium in which it occurs." Even then, mere morphology will often entirely fail us unless we are able to ascertain the physiological characters. And even these appear by no means constant; for we shall in the present paper see reason to believe that one and the same bacterium may differ at different times in its fermentive effects in one and the same organic solution." I ask anybody what can be made out of this which will accord with the notion of anything like distinctness of species? It seems to me it is subject to this interpretation, that you have the lowest kind of living matter taking on different forms in accordance with the different media in which it exists. It takes on one form in one medium, and if the medium undergoes change, it then takes on another form. So long as the medium remains the same it "breeds true," as it is said—that is, it reproduces its like, because the reproduction of these organisms is nothing more than a process of discontinuous growth. It is growth, not reproduction in any complicated sense of the term. Therefore if you bring back the medium to its original condition, what more natural than to suppose that the bacterium itself should also revert to its original condition? In fact, I may say that there does exist an enormous amount of evidence to show that all the forms depicted upon the diagram before us may slide into one another—that you may get all sorts of gradations between the torulæ and the typical bacteria. Torulæ may grow as torulæ indefinitely, or under other conditions they may grow more continuously and branch out into fungoid filaments; or some of these bacteria may elongate into more or less divided filaments, when the rapidity with which the process of segmentation occurs is lessened. You may produce these forms at will. If you take an ordinary organic infusion, expose it to a warm temperature, and leave it alone, it will produce these ordinary rod-like bacteria. If you add a drop of acetic acid, you will get bacteria of a larger size; add two drops, and changes take place less rapidly. The organisms, too, instead of multiplying as they did, now grow continuously into filaments. This does not rest upon my observation alone; such observations have been made by numerous observers. In fact, this view of the interchangability of these organisms has been supported in very able papers by M. Trécul, of Paris, who holds a reputation equal to that of Professor Cohn. If, therefore, upon the ground of mere peculiarities of form I am asked to believe that the corkscrew-like spirilla found in the blood of relapsing fever is the cause of this disease, I most certainly decline to accept that view. I should prefer agreeing with Dr. Murchison that changes taking place in the fluid and tissues of the body as the result of destitution, are the real cause of disease. I would further add that these changes taking place in the fluids of such a person are changes most favourable for the production of organisms. Then, again, there is an enormous amount of evidence which has been brought forward by Dr. Murchison and others, tending to show that this relapsing fever, in which organisms are found in the blood, is a disease which is generable *de novo*. We heard Dr. Crisp also express a strong belief that splenic fever—another disease in which organisms are found in the blood—was also generated *de novo* under the opposite condition—that is to say, under the condition of plethora or overfeeding; and it is only by ignoring the possibility of an independent origin for this and for some other diseases that Dr. Sanderson finds reason for his hypothetical views as to the "latent life" of contagion. Just as typhus may be generated by overcrowding, so it would appear that relapsing fever and splenic fever may be engendered under other conditions. But typhus is a fever in which it so happens that organisms are not engendered. Relapsing fever and splenic fever, on the other hand, are diseases in which the changes in the blood are such as to favour the appearance of organisms. Nor is this all. There is other evidence of the same kind to be brought forward. It is a well-established fact that one of the most fatal diseases ever known to occur in silkworms (muscardine) can be generated *de novo*. This fatal disease—a disease in which fungoid elements are found in the blood, and growing through the tissues of the body—elements, too, not at all dissimilar to some of the elements in ovine small-pox—may be produced quite easily by shutting the silkworms up in a glass bottle or box, and by overfeeding them for a little time. Under these conditions the blood soon swarms with these organisms, and the fungus grows through all the tissues of the body; and, when thus affected, the animal is capable of spreading the disease to thousands of silkworms with which it may be brought into contact—for it is an eminently contagious disease. And if we look at sheep-pox itself, the facts seem irreconcilable with the notion that the organisms found in the diseased tissues are themselves the cause of the disease, for after the first local contact of the contagium with some surface it is supposed to be disseminated through the body by means of the blood. But, on the other hand, it is a well-attested fact that inoculation with the blood of an animal suffering from ovine small-pox does not produce the disease in another animal. How can those who believe in the germ theory for ovine small-pox account for this? With facts like these before us, then, we are asked to believe in a germ theory of disease, because the phenomena of specific fevers are supposed to be alone capable of explanation by reference to the multiplication of hypothetical organisms within the body. But, as Dr. Murchison has already said, it seems that the facts in question are equally capable of being explained in other ways. At the last meeting we heard Dr. Maclagan's attempt to support a theory of this kind on grounds which, as it seems to me, were not a little fanciful, if not actually erroneous. Febrile heat surely cannot be accounted for by the growth of the protoplasm entering into the composition of his hypothetical germs, for it is a well-established fact that the growth of living matter is synchronous with the disappearance, and not with the production, of heat; and I thought it was also a well-attested fact, pathologically, that the heat in a febrile case was due to the increased disintegration which occurs in febrile processes. But why should I further attempt to criticise Dr. Maclagan's remarks? It would be, perhaps, ungracious in me to do so, seeing that he admits my two main propositions. He admits, on the one hand, that I have established the position that bacteria are pathological products; and on the other, that "if any advocate of the germ theory ever thought that the connexion was causal he must now give up the idea, for if the question is to be discussed by evidence we must acknowledge that Dr. Bastian has proved that no such relationship exists." What more can I wish, at all events? Turning now for a moment to virulent inflammations and their sequelæ, I may say, in reply to Mr. Jonathan Hutchinson, that I used the term "virulent" advisedly. I deliberately used that term, rather than the term "infective," because I thought that notions concerning infection are at present being ridden to death. I think that multitudes of processes are put down to infection which, in the strict sense of the term, have no right to be placed in any such category. These doctrines and modes of expression have been based upon the result of experiments with rodent animals, whose lymphoid tissues seem to be preternaturally prone to overgrow. The results, too, are commonly referred to specific infections, even though it is found that in a certain number of cases a precisely similar result may be produced by non-specific irritants—the mere introduction of a seton, for instance. Then, again, with regard to the facts of coexistence cited by Dr. Sanderson concerning erysipelas and diphtheria, I see nothing in these facts at all more favourable to his view of the matter than to that

of those who take the opposite view. They are, after all, mere facts of coexistence, and with the evidence which I have already laid before the Society on the subject, I am disposed to look upon these cases as typical cases of what I should call bacterial degeneration—that is, that the organisms which are found there are consequences of previous morbid changes, and not causes of those morbid changes. I will not detain the Society any longer at this late hour. I will only say that there is a practical outcome of such views as those which I have been endeavouring to place before the Society. Tying ourselves down to no exclusive theories, these views teach us, whilst accepting the facts of contagion, not to be pure contagionists, but rather to look abroad and around us and seek for the conditions of origin of contagious diseases. And, as I have elsewhere said, the ravages of typhus in our crowded cities and our gaols have been enormously curtailed, not so much because of its diminished spread by contagion, but rather because we have learned what are the causes which engender it, and are therefore better able to prevent its occurrence. Let us strive therefore to acquire a similar knowledge concerning other specific contagious fevers, in order that we may as far as possible oppose the conditions which favour their origin—we shall thus endeavour, in the most efficient manner possible, to check the ravages of these now almost ever-present pestilential diseases.

A vote of thanks was passed to Dr. Bastian, and the meeting adjourned.

OBSTETRICAL SOCIETY OF LONDON.

THE discussion on the Relation of Puerperal Fever to the Infective Diseases and Pyæmia was resumed on May 5th.

The PRESIDENT.—It will be in the recollection of the Society that the discussion commenced by Mr. Spencer Wells was adjourned at the last meeting, on the motion of Dr. Barnes, seconded by Dr. Squire, and it will therefore be continued this evening. Before the discussion commences I may state that Dr. Matthews Duncan has forwarded a letter of explanation in reference to a letter read at the last meeting. He understood that adverse comments had been made on his published letter, and he is very desirous of making some explanation regarding it, which I am sure will be interesting to the meeting.

The letter was as follows:—

"*To the Secretary of the Obstetrical Society of London.*

"Edinburgh, May 3rd, 1875.

"MY DEAR SIR,—As I think my former note to you was too short, I beg now to supplement it to a small extent. One of the most important points for discussion by the Obstetrical Society at the present juncture is the conduct proper to midwifery practitioners with a view to avoid disaster to patients from puerperal infection, and I expect much advantage from our great Society taking up this difficult question. At the same time, I feel called upon to deprecate premature decision, for the matter in both its scientific and practical aspects is not ripe for more than intelligent, prudent, and fearless discussion.

"Observing recent trials of our humble and unlicensed female fellow practitioners, I think there have been precipitancy and rashness, believing as I do that Mrs. Dymond and Mrs. Marsden have been severely and probably unfairly dealt with. I judge from the meagre reports regarding the cases of these midwives in the weekly medical journals. The charge of homicide by infection is, I believe, a new one in the history of the law, and I am further of opinion that in the present state of science and practice such a charge cannot be substantiated. I know that Mrs. Marsden is now in prison enduring penalty for homicide by infection, and that this is an example of the charge being sustained; but while I grieve for this sufferer, I feel sure that the case would have broken down had it been tried in a proper manner.

"The public, and to some extent also the professional, mind is not well informed on the subject of puerperal mortality and puerperal infection, and there is much excitement abroad regarding the prevention of disease generally. Under these circumstances, error is easily fallen into, even by authorities, when brought face to face with the terrible facts probably for the first time.

"I shall say nothing about the amount of puerperal mortality, presuming that many of the members of our Society have read some remarks on the subject in my last year's address at the Norwich meeting of the British Medical Association; but I may remind you that such high and justly-respected authorities as Miss Nightingale and Dr. Wm. Farre have promulgated far too favourable views as to the smallness of this amount, and that exaggerated notions of the innocuousness of child-bearing are fostered by the careless talk of many experienced medical men, asserting, as they are often heard to do, that they never lose a case. If it is believed, as these would have it, that there are very few deaths, it is easy to understand the panic excited by the actual facts.

"On another point there seems to be still greater misunderstanding on the part of the public, and of the profession—namely, the precautions taken, or considered necessary by the profession to avoid communicating disease to patients. For the prevalence of this error I can cite the evidence of *The Times* newspaper, whose utterly erroneous statement I now give as I find it in a leading article of that great organ of public opinion:—'It is the invariable practice of medical men if they attend a case of this (puerperal) fever to hand over the whole of midwifery practice to other persons for at least two or three months, and it has been shown by ample experience that this course is absolutely necessary to preserve the lives of their patients.' Anything more inconsistent with truth would be difficult to find. It is this error, thus widely promulgated, and no doubt entertained, that I wish to point out; but I may be allowed to add that were this true I would never have a chance of being in practice. I would be perennially on the shelf, and so would everyone who had a large practice, especially if that practice involved hospital duty and private consultations.

"The difficulties of determining the proper conduct of obstetricians are not to be solved by officious coroners foolishly sending threatening messages to practitioners, nor by judges giving decisions without due consideration, but by discussion in such a Society as ours, and by matured professional opinion. Were the conduct of the coroner in Mrs. Dymond's case, and of the judge in Mrs. Marsden's, to be held as good precedents, there is not one among us who would be secure for a day against the most terrible charges. No surgeon having a case of pyæmia or of erysipelas could go safely to any other patient; no physician having a case of scarlatina could go elsewhere. The lying-in woman no doubt requires special care, but such care does not differ in kind from the care of other patients. Homicide by infection may be as easily brought home to the surgeon or physician as to the midwife.

"I regard the recent prosecution of midwives as injudicious, because I believe it would be difficult to find a physician or surgeon who might not on the same kind of ground be cast into prison. The crime, so called, of Mrs. Marsden is to be found almost every week lately confessed by ingenuous practitioners writing in our journals. Moreover, I would have preferred that some *man* should have been first charged with this crime, and not a poor and comparatively defenceless woman; and, as I have said, it will be easy for the authorities to find a criminal man. Where is the physician or surgeon who has not many, many times exposed his patients to some risk of infection with dangerous disease? Is a physician to be found guilty of homicide who exposes his patients to no greater risk than is that of those dearest to him at his home?

"The medical profession with one voice proclaims the necessity of extreme care and prudence on the part of medical men, and the utter abnegation of selfish views in their conduct of practice. But there is danger of the public and of many professional men being now led to seek for and expect a degree of safety which can in no ordinary circumstances be reached, and to entertain views as to the conduct of practice which are based on error and lead astray from the line of expediency and prudence. Absolute safety can in no way be obtained. As practice is at present conducted by careful practitioners, I believe it is nearly as safe as it can be. Among the necessary precautions in ordinary circumstances I do not place giving up practice for a time, and I may say, for myself, that during nearly thirty years of

obstetric experience in private, in hospitals, and in consultation, I have not, as a precaution, given up work for a single day. And I venture to say that it would not astonish me were it demonstrated that as much disease and death comes from patients to practitioners and nurses as goes from practitioners and nurses, ordinarily and duly careful, to patients. I must draw to a close, without having said nearly all I wish to say, and merely name the following grand precautionary measures for obstetric practitioners:—1. Avoidance of the duties of nurses. 2. Avoidance of using the hands in post-mortem investigation. 3. Antiseptic cleanliness of the hands and of the dress.

"One case of infection by a practitioner is as heinous as a series of cases. One homicide is as bad as four. The chapter of accidents may bring a series of cases to one practitioner who is really careful and innocent, and the series may appear to be the result of infection. I have never had a series of cases, not even two near one another, in my private experience; and I believe such an occurrence would drive me away from my practics, whatever my theoretical views might be. Many practitioners have in such circumstances persisted in practice, and do so now, just as Mrs. Marsden did—in my opinion, foolishly, though not criminally—persist. If homicide by infection is a good charge of manslaughter, it is as good in a case of one infection as in a case of four, however appalling the latter may be. There are few practitioners in any department of medicine against whom such a charge might not be set up.

"Allow me to express my regret at forced absence from your meeting, and believe me, dear Sir, yours faithfully,

"J. MATTHEWS DUNCAN."

Dr. BARNES.—I shall be quite willing to cede my right of priority to any visitor who may be willing to address us. We who are in the habit of attending here have spoken so often that I think we must be almost tired of hearing our own voices, and one great advantage of this particular discussion I take to be that it brings into our Society the views entertained by persons who do not usually come here, especially of men practising in different departments of medicine. I have been in the habit of saying that in obstetric practice we may see at least the germs or the illustrations of almost every problem in medicine and surgery. We want, nevertheless, to have the advantage of the additional light thrown upon these problems from all sources. Of late years, owing to the enormous growth of the metrophlis, and the great number of medical men practising in different departments, the tendency to cut up medicine into sections is attended with this disadvantage, that we cannot, except on rare occasions like this, get the benefit of the varied experience of men bearing upon one particular department. We should be glad to have the advantage of hearing those who practise surgery and medicine; at the same time I think that these problems of pyæmia and fever may receive some of the most valuable illustrations in the practice of obstetric medicine. I heard the letter of Dr. Duncan with considerable satisfaction. I think it places the grave and great difficulty which oppresses medical practice before us in a very striking manner. If prosecutions are to go on as they are now doing, I do not know how it will be safe for anyone to practise at all. It is becoming a most serious matter. We are governed by the ignorance of the law and the ignorance of the judges. The public are taught improperly. Instruction on this matter, as Dr. Duncan rightly suggests, ought to come from us, and that is one of the advantages, I hope, of a discussion of this kind. The lawyers and the public must receive, or ought to receive, medical opinions from the medical profession, from experts, those who are skilled and know what they are talking about. It will be dangerous to the public at large unless this principle is carried out more fully than it is. With regard to the great question of puerperal fever, of course for many years as an old teacher I have had to consider this question, and I have been trying and struggling to put my ideas into a definite form capable of being understood and taken up by others. I have felt the difficulty expressed by Dr. Leishman at the last meeting; still, after debating the subject in my own mind, I have come to something like an outline of definite ideas, which of course I am ready to change to-morrow, or even to-night, from what I may hear, as I quite admit the propriety of holding the mind perfectly free and untrammeled, ready to receive any kind of opinion, from whatever source it may come, and especially to study facts and the bearing of those facts. The leading idea, which tends to simplify the investigation of the subject, is one which I took some pains to illustrate some years ago, in a series of lectures published in THE LANCET, but never finished—that is, that we may, at any rate, divide the cases of puerperal fever into two great classes, and having got as far as that, we can proceed a little further with the analysis, and perhaps pursue each series of cases ultimately to their true source. I think we may lay down this proposition, that we cannot discard the term puerperal fever. Notwithstanding the respect I have for my friend Mr. Hutchinson, whom I look upon as one of the most enlightened pathologists of the day, I cannot see my way to discard the term. The meaning attached to it is simply this: fever in a lying-in woman, the general term expressing a number of perfectly different conditions. Having admitted that simple meaning, fever in a lying-in woman, we may proceed to analyse it and to trace the different varieties. I think we may here divide it in this way. We must all of us see examples of it every day. There is the form of fever in the lying-in woman which is the direct result of infection or contagion produced by some zymotic poison, as scarlet fever (perhaps the most common of all), or erysipelas, or measles, or typhoid. All these things we see and know, and we cannot for a moment dispute them. Well, then we have a large class of cases, perhaps the most numerous, which we may call heterogenetic, which have arisen outside the patient's body, and have been put into her. Then there is another class of cases, which may, perhaps, admit of more dispute—a class of cases which I have called autogenetic,—in which all the conditions of the fever exist or arise in the patient's system, with which infection or contagion from without has nothing to do. That cases of this class exist I am as convinced of as I am of the cases of scarlet fever; they are manifest to everyone. But the objection has been raised, and has occurred to most of us, how is it that lying-in women are especially prone to scarlet fever? Like other adult persons, the great majority of them have had scarlet fever at some previous period of their lives, and may be supposed to be more or less protected; and how is it that their protection all of a sudden breaks down under the trial of childbirth? It is said again that scarlet fever can give nothing but scarlet fever, just as an acorn can bring forth nothing but an oak. That may be true to a certain extent, but women in childbed are in a peculiar state of susceptibility to any poison; and the specific course of scarlatina may be masked in them. I have seen cases traced to scarlatinal poison in which the usual symptoms of scarlet fever were absent—no particular sore-throat, no swelling of the glands, no rash,—and yet the cases have gone on to a fatal issue. Then, if we go a little further, we see what was alluded to by Dr. Richardson—who was, I think, the only speaker at the last meeting who hit upon the right key. If we look at what a lying-in woman is, we there see a peculiar constitution ready to receive poisons, and ready for those poisons to ferment and go on to a disastrous issue; while in another case the poison has no such effect. Then there is a peculiar condition following labour, where the system has been loaded with matter striving for excretion: there is the involution of the uterus, the discharge of superfluous blood, the milk process coming on—a state which is just treading on the verge of fever at any moment,—the slightest excitement or the slightest noxious matter carried into the blood is ready to ferment and set up a fever. It does not matter what the poison is. I think that Semmelweis, in his investigations, showed that even a case of cancer in a ward was the starting-point of a series of puerperal cases. It may be said that a cancer-germ will produce nothing but cancer. It will produce fever in a lying-in woman. So will scarlet fever. This peculiar constitution is one which we may recognise in the range of surgery and medicine as well as obstetrics; although in obstetrics we get the most striking illustrations of this as of many other pathological forms. In surgical practice, for example, we may see sometimes persons, adults especially, who cannot be vaccinated with impunity. We all know cases of that kind. The blood is in a ferment at once. This simple, mild, laudable poison, as we may call it, vaccine lymph, will set up a ferment, and the patient may die. Sir Benj. Brodie used to tell us of a man who was killed by the sting of a bee, so great was the irritation set up by the poison; but that argued a peculiar state of the system. We know there

are persons who cannot scratch themselves without a fester: all going wrong in a moment. Well, a similar condition exists in an exaggerated degree in the lying-in woman, no matter what the poison is—scarlet fever or measles, or anything else. In these, the heterogenetic cases, the symptoms are manifested earlier than they are in the other class of cases where the poison arises in the patient's own system. The fever breaks out in twenty-four hours or in two or three days, whereas in the other cases it generally comes on later. With regard to scarlet fever, it is enough to set up any mischief in a lying-in woman, and produces all the mischiefs of any other form of poison. We all know that scarlet fever poison, whatever pathological change it produces in the woman herself, can propagate scarlet fever to others. Then I would pass to the autogenetic cases. These cases are as distinct in their origin as many cases of infection. For example, you see a woman in the country away from all sources of infection; a little bit of the placenta is retained, a clot of blood is there, or some change takes place in the uterus, and there is an element of infection; it runs along the veins or the lymphatics, is absorbed by the mucous membrane; then you get the blood tainted, and the slightest matter will set it going; the whole system is in a ferment, just as it was from the poison of scarlet fever or typhoid; you may call it pyæmia or septicæmia, the result is about the same. These cases come on a little later than those which have a zymotic origin, and they can often be arrested by washing out the uterus, and bringing away any superfluous matter there. Many women will succumb at once or rapidly to a single dose, no matter how small it may be, but others can resist to a certain extent; their excretory organs may be in good working order, and they may throw off a moderate dose or two moderate doses; but they cannot survive repeated doses. If you can wash out the uterus, you may prevent the renewal of the poison, and stop the disease. That principle has recently come into vogue again. It was practised and taught with success by Harvey, who, if he had not been the greatest of physiologists, would have been perhaps recognised as the foremost obstetrician in the world. This mode of infection is one of very great importance for us to consider. There was a case referred to by Dr. Huntley, who believed that infection was taken into the medical attendant's system, and might be given off by the skin. I believe it may be propagated by the breath of a medical attendant or a nurse. We must all be conscious sometimes of taking in poisons by coming into contact with poisonous patients. I have gone away from a craniotomy case, where the brain was foul, stinking of it, my breath smelling for a day or two. So with other diseases. So with dysentery, I had shivering, diarrhœa, and foul breath, from the odour of dysenteric stools, for two or three days after being in contact with a patient of that kind. A man may walk about charged with infectious disease, and those who are susceptible with whom he comes in contact may catch it; those who are not may perhaps have a little dose, which they can throw off, the system being in good working order, and there is an end of it. But a lying-in woman, with the blood ready to ferment, would be readily attacked. There is the secret of the difference. A medical man who has seen a case of scarlet fever comes in contact in the course of the day with twenty or thirty patients, and it is perhaps only the lying-in woman who takes the disease; not that he is necessarily longer with her, but there is a greater liability on her part. I had a letter this morning from the son of an old member of this Society—a former vice-president,—who calls my attention to a work by his late father, Dr. Uvedale West. He gives one example which is instructive, and I thought you would like to hear it. He had an outbreak of puerperal fever in his practice; he had to deliver a woman to whom he went straight from a case of erysipelas, and this woman had erysipelas the next day; he went afterwards to another patient, whom he did not examine, and that woman escaped; the next day he went to another case, and the woman died. That is the history of a great many of the epidemics in the German lying-in hospitals, where they have the faculty, as it seems to me, of manufacturing puerperal fever on a large scale. Those women who are lucky enough (we should consider them unfortunate) to be confined in the streets, on their way to the hospital, escape puerperal fever; it is only those who are examined repeatedly that catch it. That has been observed over and over again. If I might be permitted a moment or two longer, I could give you the history of a series of cases occurring in the practice of one midwife in a short time, while all the surrounding neighbourhood was at the time pretty free. I will now only sum up my conclusions in reference to the questions submitted by Mr. Spencer Wells. "Did you ever see a case of puerperal fever which is not really a case of scarlatina or rubeola, or erysipelas or traumatic fever caused by the bruising or tearing of parts?" I do not think there is any fever caused simply by bruising the parts. If there is a little scratch, no matter how small, and the poison is conveyed to it, that is a different case. It is not necessary to suppose that there is a puerperal fever which runs a definite course like scarlet fever. We get a fever which is fatal, and that is serious enough to be considered as a case of puerperal fever. As to the question whether there is a real form of puerperal fever which is not scarlatina or any of the conditions specified, I will say that there is, and I have called it excretory puerperal fever. We have albuminuria at the end of pregnancy, and those cases are extremely apt to go into puerperal fever. The blood is peculiarly overcharged, the liver and the kidneys cannot act, excretion is at an end, and then the poison runs riot without any ingestion of poisonous matter from without. That is pure puerperal fever which the patient herself can generate. Then, as to how the spread of the disease can be prevented. I can only say by careful isolation of the patient. The conditions of practice are sometimes incompatible with that. There was a case mentioned by Dr. Uvedale West. He was on the point of going away, and there was no one to take his place. The patient sent for him, would have him, and she fell a victim. That may happen to anyone in the country. He cannot get away when he likes. With regard to the question of bacteria, as I know nothing about it, I had better say nothing; but I may reserve my doubt with Dr. Richardson, and wait till the bacteria doctrine is proved by those who understand it. With regard to the value of antiseptics in order to keep hospitals free, I think that to keep hospitals free from puerperal fever is an extremely difficult matter—more difficult than it is in a surgical hospital to keep it free from pyæmia. You cannot keep a series of patients in a hospital isolated, in the proper sense of the word. You have the same nurses going to them; you have a variety of influences acting upon one or two patients, and the consequences may be radiated to others. There is only one secret for safety, and that is to have the woman confined at her own house, where she can have her own nurse, who has not been in the way of infection, and her own medical attendant. Then the chances are that she will go on favourably and happily. Without that there is no security. A lying-in hospital is not now by any means so serious a danger as it used to be; still it is always like a volcano, which may explode at any moment.

Dr. SQUIRE.—I am sorry that the limitation of time has made the valuable observations of Dr. Barnes a little hurried. I must say that, agreeing with him so much, I was a little surprised to hear him support a statement of Dr. Richardson's as to the very febrile condition of lying-in women—an idea which I thought was derived from the depths of his inner consciousness, and not as the result of bedside experience. In the course of a very large inquiry I have failed to find that febrile disturbance in the majority of women in well-to-do circumstances. This important subject was ably brought before us for discussion under two divisions. It is to the queries under the first of these divisions that I wish to reply; and it is to the second question that I intend to confine myself more particularly. I presume that "there *is* a fever after childbirth, caused by a morbid poison communicable by infection"; and while admitting a large class of infections associated with puerperal fever, I hope to show that the whole class of acute specific diseases may be set aside. It may be one extreme of error to deny the existence of puerperal fever; it is certainly the other extreme to call every post-partum illness by that name. Almost any febrile attack in the puerperal state will cause suppression of the lochia, arrest of the lacteal secretion, and other common symptoms; so that we have to separate many febrile ailments, as well as what may happen from retained placenta, or other causes of metritis and phlebitis, from the disease under consideration. Our first duty in meeting with such cases is not to call them by the name of puerperal fever, but to diagnose their

nature. Many of them we can soon relieve, and in all be sure of carrying no infection to others; while no case of puerperal fever is without this risk. Question 2 deals with the relations of puerperal fever—first, to the specific infective diseases; and, secondly, to the traumatic fevers and septicæmia. Clear as was the introductory statement, I noticed that, in repeating the question, Mr. Wells transposed erysipelas from the second category to the first. I do not mean to deny it a place there, but for the present purpose prefer to keep to the terms of the original proposition. I am then able to answer the second part in the affirmative, and to give to the first part a denial as precise and distinct as possible. I go beyond this, and say that no form of puerperal fever is to be referred to attacks of the specific infective fevers. The Transactions of our Society, abounding in excellent materials for the subject in debate, furnish more commentaries against the view I take than in its support. In the very carefully prepared paper in the third volume I find it stated from the chair that "any of the agents which produce zymotic maladies might cause puerperal fever." And again in vol. x., the president, among remarks of high value, says, "it can be caused by the contagion of typhus, measles, and scarlet fever." At the last meeting this opinion seemed general. This view has led some to look upon this disease as merely the puerperal form of scarlet fever, or to consider it typhus. This view I controvert, and assert that not only is puerperal fever not typhus, typhoid, small-pox, measles, diphtheria, nor even scarlatina, but that these diseases are little modified by the puerperal state and retain their distinctive characters, so as to be recognisable; they ought to be diagnosed under this, as under other conditions, and called by their own names, and not by that of puerperal fever. Moreover, though pregnancy and the puerperal fever may prejudice the prognosis in such complications, and abortion happens in some of them, yet the puerperal accidents are not always so grave as is supposed. I will first take typhus. Of 1000 typhus patients admitted to the London Fever Hospital 16 were far advanced in pregnancy; they miscarried, and the majority made good recoveries. This is very different from the results of abortion in typhoid, where, however, the fatal result is owing to the serious importance of this accident to the fever, and not to the development of puerperal mischief. Though it is quite possible for puerperal fever or purulent infection to occur after abortion, even in the earlier months, yet we see nothing in typhus to cause puerperal fever. But I have a good authority against me. Sir Henry Marsh, in the fourth volume of the Dublin Hospital Reports, says, "the true character of epidemic puerperal fever seems to be typhus." This is founded on the experience of Dr. Johnston, Professor of Midwifery to the College of Surgeons of Ireland, who states "that the ward-maids of the Dublin Lying-in Hospital caught typhus from patients received there." Typhus was prevalent at Dublin, and this shows how typhus may find its way into a lying-in hospital. Twenty years ago there was typhus in Leeds; a friend of mine attending to midwifery in the Leeds School of Medicine met with a case of typhus at childbirth; the fever was well marked, it commenced before delivery, and the characteristic mulberry rash occurred; it was not communicated to any other of numerous patients consigned to his care, though some cases of puerperal fever were met with in the practice of other students. It is not contended that small-pox is modified by the puerperal state. Vaccination saves us from meeting with it in the severe form; but I have one instance where it was easily recognised, and did not produce puerperal fever. When I saw the patient she was in severe pain; she had gone her full time; she looked ill; and the temperature was 104°. That being utterly different from anything I had found in an ordinary case of labour, and being what occurs in no other ailment besides, I was led to question what happened. I found that a young man sickening with small-pox had been removed from a house in which she was. The next day labour pains came on, and in the night she was delivered. The spots of small-pox came out on the following day. The child was removed, and vaccinated at the same time. The woman made a good recovery, without any puerperal symptoms whatever. Now if a poison of small-pox can produce puerperal fever, that was a case in which it ought to have occurred. In measles it is rare to find parturient women unprotected by a previous attack; but when so protected, I have seen a mother nurse her child in measles without the least injury up to the time of delivery. Lately I saw such an instance, where the poison must have been concentrated in the room; for the baby had a rash a fortnight after birth, the mother keeping quite well. Therefore the statement that measles will convey contagion to parturient women seems doubtful. I will quote one instance from the "Sydney Papers" of measles at parturition in a woman. Lady Sydney was sickening for measles, when, on the third day, with severe cough and full rash, "she is brought to bed of a goodly fat son." The child was also full of the measles, mostly in the face, yet it sucked the nurse as well as any child could. Both did well. With respect to scarlet fever, no one has a more wholesome dread of it than I. Every means of isolation should be used to keep it out of families. Once it happened to me to see in the family of a physician, who devotes his time to the study of how to prevent disease, scarlet fever in a little girl, whose mother had just been delivered. Not only were there no ill symptoms, but not a trace of puerperal disease, but the progress was favourable. The mother had never had scarlet fever before, yet it is quite certain the infant took the disease and did well. Dr. Braxton Hicks, in his admirable paper in the twelfth volume of our Transactions, gives me the most valuable support by his cases, though we differ in the conclusions to be drawn from them. Of his eighty-nine cases, but one is entered as true puerperal fever, and of the thirty-nine cases of scarlet fever there was no difficulty in the diagnosis, twenty of them having well-marked rash. The great point demonstrated by these cases is the special liability of puerperal women to scarlet fever. Infection is resisted up to the time of delivery, then they succumb to three or four doses. Diphtheria has given me much anxiety, and, to my surprise, soon after delivery the exudation, instead of extending, had cleared off, and an obstinate ulceration was healed. On turning to our Transactions for a case of post-partum diphtheria, I find it headed "Mild Puerperal Fever." When we come to erysipelas, the case is very different. Dr. Rigby noticed that the children born during an epidemic of puerperal fever had erysipelas. I do not mean to say puerperal fever is erysipelas, but this shows its connexion with that and hospitalism and purulent infection, and probably cancer. I believe we can connect this closely with that class of infectious diseases which the investigations of Billroth and Lister have not only enabled us to understand, but to control. I believe there is danger in those dissecting attending midwifery cases. I have seen a dissecter suffer from peritonitis after operating on a bad part, and I believe he might have been the means of conveying infection to those whom he attended.

Dr. BRAXTON HICKS.—A remark fell from Dr. Squire with respect to the rash accompanying local cases of scarlatina. I think he will find a large number of such cases. Certainly my observations have gone to show that there is no rash, and that there are none of the permanent symptoms of scarlatina where scarlatina has been unquestionably mixed up with the case.

Dr. BRUNTON.—Mr. President and gentlemen: I feel called upon to rise to make a few remarks upon the subject now under discussion, because, as a general practitioner, I think that this subject of the power of carrying infection from one patient to another is of vast importance to all who are in the habit of attending all sorts of infectious diseases and at the same time attending midwifery cases. If the poison of contagious diseases is so productive of puerperal fever as it has been pronounced to be by many of the speakers in this assembly, I cannot help saying that in my own practice I have entirely failed to find it such. The reason for it is this. I come forward as a practical man. Theory is all very well, but when we come into general practice one must look at what is seen in every-day work. The first midwifery case I ever attended was in the small-pox ward of the Royal Infirmary, Glasgow. The patient was in the full bloom of small-pox, and aborted about the seventh month. She recovered without a puerperal symptom of any kind. Then, as Dr. Leishman has said, the students at the University of Glasgow, during the time they are dissecting, are in the habit of attending midwifery cases. I did the same, and I know some gentlemen in the room who were fellow-students of mine who also did the same, and during the whole time of our practice we had no puerperal fever at all. Then I came to practise in London, and I went to

see a lying-in woman who had two children lying ill with scarlet fever; she made a good recovery, with not the slightest feverish symptom whatever. Then, again, general practitioners are in the habit of making post-mortem examinations. They meet with cases of sudden death, and the coroner calls upon them to perform post-mortem examinations. I have done it over and over again. I have gone on with my obstetrical practice all the time, and not had in the whole course of my practice a single case of puerperal fever. I saw one case of scarlet fever many years ago where the patient went through the full course of the disease with very putrid discharges as offensive as could well be, and yet the patient recovered. The skin peeled off; she had the usual affection of the kidneys and so on, and yet did well. My evidence in this discussion is decidedly negative. It appears to me that if scarlet fever, typhus, measles, and small-pox are to be reckoned such very strong producers of puerperal fever, I ought to have had a great deal of it in my practice; but that has not been the case. I will read a note that I received the other day from Dr. Caskie: "A few months ago I was sent to see Mr. P——. I found him covered with the scarlet fever rash, and by his side his wife in bed, three days previously confined. Her labour had been natural. The husband was taken away, and she made a good recovery. Her own children escaped, but the children of the landlady in the house took the fever, and two of them died." Now, if scarlet fever in that case was so deadly, Mrs. P—— ought to have taken it, and passed through the usual course, or died. I cannot help referring to the remark made by Dr. Richardson and Dr. Barnes about the peculiar physiological condition of lying-in women. It is said that at that time the blood is in a peculiar condition. Certainly that is the case, but I look upon it from another point of view. While there is an excess of fibrine, and a diminution of blood-corpuscles, and a diminution of solids and an excess of albumen, I look upon this as nature's preparation for the casting off of the ovum; I look upon the excess of fibrine as nature's safeguard against hæmorrhage. I also look upon the excess of fibrine as one of the means for the easy production of the lacteal secretion to follow. Mention was also made of the feverish condition that is said to follow the immediate birth of the child. Since the last meeting I have had the opportunity of speaking to Dr. Richardson, and I have made observations on this point, and in only one case have I been able to find any increase of temperature worth speaking of, except in one case, which was not attended by myself, and in which the lady desired not to suckle her child. There, while the pulse remained at 72, the temperature was as high as 104°; the breasts were very hard, showing some sort of rapid tissue-change at all events. What appears to me to be the nature of puerperal fever, as far as I can theoretically make out, is this, that it is simply autogenetic; that it begins in the uterus of the female who has very lately had hæmorrhage, or is weakly, and there is an imperfect contraction of the uterus; a clot remains, that clot decomposes, and then you have pyæmic poison. Then I do not deny that the poison coming off the excreta from the woman in this pyæmic condition may be infectious to others. I regard such a woman as being in a similar condition to one who has had a thigh amputated. We know that if pyæmia occurs after such an operation the dressers are not allowed to dress the wounds of any other patients. No doubt, as Dr. Barnes has said, during this process of pyæmia, the poison may be absorbed and then excreted by the lungs of the individual who is in attendance, and be carried to another patient, and there set up a similar form of disease. While Dr. Barnes was speaking I came to the conclusion, from what he said, that if the contagious diseases are so productive of puerperal fever, and if the medical man is a focus of infection by exhaling the poison, the judges at the present day should look upon a man as a criminal if he attends obstetric cases while he is attending a fever case. I must say that my experience is exactly the opposite of that.

Dr. HUNTLEY (of Jarrow).—Mr. President and gentlemen: In responding to your invitation to strangers, I confess I feel some diffidence, in part arising from the fact that Dame Nature has not endowed me with that glibness of tongue which stands its possessor in such service on occasions of this kind, but chiefly arising from the fact that I am addressing many of the most distinguished members of our profession; and it would ill become me, a stranger and of no repute amongst you, to attempt to dogmatise. But as my experience has been somewhat peculiar, of a nature that does not occur, perhaps, to many medical men (perhaps not a single member here present has had a similar experience), I may be allowed to give rather freer expression to my opinions than otherwise I might do. I will briefly describe to you the experience to which I refer. About ten years ago (in December, 1864) I had an outbreak of puerperal fever in my practice. I was then a very young man, and the anguish of mind which it caused me was something which I shall never forget. In the course of two months I attended fourteen cases of fever, five of which died. All the cases that I attended were not affected by the disease; at intervals two or three escaped. However, at the end of two months I think two patients died. Then I was obliged to cease practice, having come to the conclusion that the disease was in some measure connected with myself, for although there were four other practitioners in our district, not one had a case of the kind. I was forced to the conclusion that coincidence could not account for such a continued sequence of cases, and that I was the cause of propagating the disease. But how could I propagate it? That was a matter that exercised my mind for many a day. We had small-pox very prevalent at the time in our midst, but no other infectious disease. I was then attending a case of severe burns with great suppuration, but that I only dressed for a few days, and I think it is very unlikely that the infection could be carried from such a source for two months continuously. If it were so, I think such instances would be very frequent, and I could not but consider it absurd in the extreme to think that for weeks, considering all the precautions which I took, frequent ablutions and change of clothes, I still kept the disease. It seemed to me then, and I have no reason to change my opinion now, that the disease was somehow or other reproduced, and the only source from which I thought it could be reproduced was from some poison generated in malassimilation, or in some defect in the secretory or excretory system. Arguing from analogy, I see no reason to doubt that many infectious diseases are propagated in this way, and not from clothing. Suppose a nurse is attending a case of scarlet fever for weeks, and does not have the germs of scarlatinal poison entering her system and being exhaled from it, we cannot suppose that the disease will be communicated. There is no proof at all that the clothes communicate the disease always. Perhaps there are instances in which they do; in the case of washer-women it has been communicated in that way; but I think it is just as reasonable to suppose that vitalised material will be allied with the poison as unvitalised. But it was not chiefly to relate this experience that I came here to-night. I had an impression from reading the report of the last meeting that opinions were expressed which were very far from correct. The opinions I refer to are those chiefly discussed by the two previous speakers. I think it was Dr. Braxton Hicks who asserted that scarlatina was a very prolific source of puerperal fever. But in tracing infection in such a way you are very likely to drift into error; for if you approach the subject with any fixed opinions, the wish is apt to become father to the thought, and you are brought to a conclusion not in accordance with facts. I have never in my own practice been able to associate scarlatinal poison distinctly with puerperal fever. On the 12th November last I was attending two severe cases of scarlet fever occurring in children. The mother of these children took scarlet fever, and the symptoms in her case differed not in the least iota from those of the children. I can call one or two other instances to mind where such has been the case. So with small-pox. I have seen two or three cases of small-pox in parturient women, but they have always manifested the characteristic eruption, and there could be no doubt about stamping them as small-pox. In the case of scarlet fever to which I referred I may say that the woman had no symptoms of inflammation of the uterus, or anything at all to distinguish it. I think what we should really do is, not not to argue from a particular case to a general conclusion, but to take general facts and deduce particulars from them. I regret to say that the returns of the Registrar-General are not useful in this respect in assisting us to arrive at a satisfactory basis. The returns of puerperal fever, I think, are annual, and extend to counties. If these returns were given quarterly and in particular districts we should be able

to see whether scarlet fever occurred synchronously with puerperal fever, and then we should have a safe guide.

Dr. BROWN.—About six weeks ago I was engaged in attending a patient in her confinement. At that time a child in the house, her own house, was taken ill with a pretty severe attack of scarlet fever. She was under great apprehension at the time of her confinement, expecting daily that she would suffer from scarlet fever. About a fortnight after the rash came out on the child she was confined, and did perfectly well, without a bad symptom. The infant had no sign of scarlet fever. About ten days after that another child in the house had scarlet fever badly. I attended another similar case about two months ago. The patient had never had scarlet fever herself; she was quite positive of it. In another case I attended a woman with her own child ill with the scarlet fever in the house. For some days she was laid up for her confinement, and she got up without a bad symptom. She had never had scarlet fever. I may mention that I attend on an average three or four midwifery cases a week, and I have never had a case of scarlet fever in which I could say that I had conveyed the disease to the patient.

Dr. SWAINE (of Clifton).—I think the letter read at the beginning of this meeting is calculated to do great good, especially as regards the unfounded statement that appeared in *The Times* that medical men could retire from practice for two or three months. The statement has in some places created a panic that is becoming quite a nuisance. I will give an instance that occurred to myself. About the 16th of March last I consented reluctantly to see a bad case of puerperal fever. I listened to the solicitations of the husband, but I only saw the patient once. A gentleman whose wife I had engaged to attend at the beginning of this month happened to hear of it, and wrote to me requesting me to release him from the engagement, which of course I did. She is not yet confined. As to the mode in which the infection of puerperal fever is conveyed, I think it is generally conveyed by the person of the accoucheur more than by the clothes or any other way. Some men are peculiarly unfortunate in this respect. It is often observed that all the puerperal cases in a district are limited to a few practitioners. It is not only that they have the run of them at a particular time, but even after a long interval it is the same men who get them. Some men, I believe, have the power of absorbing and exhaling these poisons to a much greater extent than others; and from remarks I have made in my own experience, I am inclined to think that the poison is much more likely to be given off by the skin than by the breath of the practitioner. I have come to the conclusion that men who have very moist, perspirable skins, especially moist hands, are much more likely to exhale it than those whose hands are generally dry and cool, especially if they have to make frequent examinations during labour. With regard to the precautions to be taken to prevent the spread of puerperal fever, I cannot think that it is necessary for a medical man to exclude himself for more than a week at the outside from midwifery practice. If he is unfortunate enough to have a bad case, probably by that time the poison will have passed out of the system. He should also take the precaution of not wearing the same clothes. I am not aware of having conveyed anything of a puerperal kind to a patient except once. That was in a case of scarlet fever. As a general rule I now refuse to go to a case of scarlet fever, unless of course it should occur to a lying-in woman. I refuse to see children with scarlet fever on account of the danger of conveying it. Some years ago I did not do so. I was attending a child with scarlet fever, and I attended a lady at the same time. About a week afterwards the rash came out, with sore-throat; she did perfectly well, and did not show any symptoms of puerperal fever. But the worst case of scarlet fever I ever saw was in a lady who, about a week before her confinement, called at a house where the fever was; the children were ill, and she was afraid to go in; but most imprudently the mother came out directly from the sick room to report how they were going on, and she put her head into the carriage to talk to this lady. In that case the scarlet fever came on *pari passu* with the labour. I attended her about 10 o'clock at night. I observed that her face became red, and at the latter part of the labour the speech was rather muffled. The redness of the face did not excite particular attention, as it generally comes on at the second stage of labour. On the next day I found she had been delirious; she was covered with a thick rash, had a sore throat, and died in two days. Immediately after seeing a case of puerperal fever I go home, and before going to bed take a warm bath and wash myself with carbolic soap; and on the next day I take a Turkish bath, which I think is an excellent way of eliminating the poison and cleansing the skin. I need hardly say that, with every accoucheur personal cleanliness is of the greatest possible importance. We know that cleanliness is next to godliness. We cannot be too particular about it, especially after seeing cases of this kind; and we should be very careful not to wear the same clothes. With these precautions in a very few days there will be little or no danger of conveying the poison from one patient to another.

Dr. GRAILY HEWITT.—The subject which is now before the Society is one which has on many occasions interested me very deeply. I have seen a great deal of this disease in public and in private, and it has been my duty not only to learn what the disease is, but to endeavour to combat it, and to teach others in the best manner I could how to deal with it. I think this discussion is extremely opportune, and that it is likely to lead to very good results. Our thanks are due to the President and to Mr. Spencer Wells who have endeavoured to bring the matter before this great Society. Now I will not dwell further on preliminaries, but go at once *in medias res*. The opinion which I have formed with respect to puerperal fever is that it is essentially a form of blood-poison. Some seven or eight years ago I read a paper before this Society in which I detailed the experience which had occurred to me in the British Lying-in Hospital with which I had been connected for some time. I brought forward a considerable number of cases, and in giving my conclusion in reference to the nature of the malady, I expressed myself in these words:—"It is impossible to escape the conclusion that puerperal fever consists in nothing more nor less than an injection into the general circulating fluid of a poisonous material of animal origin; that it is a form of pyæmia for the production of which the minutest portion of the morbific agent may prove sufficient." I was very early impressed with a lecture published by the late lamented Sir James Simpson, of Edinburgh, I think about 1859, in which he strongly advocated the doctrine that puerperal fever is a form of pyæmia; and what I had seen at that time, and what I have seen since, has certainly led me to endorse that view of the matter in the strongest manner possible. I may say that I entirely disbelieve in the existence of a form of fever which is sufficiently definite and precise to receive a distinctive name, in the same sense that we speak of typhus fever, or typhoid fever, or measles, or scarlet fever, or small-pox, each of which has a definite and well-determined course. I can see nothing in puerperal fever which at all resembles this. I think all the clinical evidence producible on the subject is entirely opposed to that view, and therefore I have no hesitation in answering that part of the question put by Mr. Spencer Wells in the way I have done. I think it would be profitable to divide the cases of puerperal fever, or puerperal pyæmia, into two classes. There are the cases in which there is very distinct evidence of the introduction into the system from without of a morbid animal poison; there are, in the second place, cases which do not resemble these, in which the evidence is wanting of the introduction from without of such a morbific poison; and I will endeavour to make my remarks under those two heads. In the first place, in reference to the cases that occur from the introduction of poison from without, after what has been said on this occasion and on former occasions, there can be no question that it has over and over again happened that the disease called puerperal fever has been produced by inoculation (the word seems to me to be suitable) in those unfortunate cases where the medical man himself conveys the disease, and in those cases where the midwives are equally efficacious in communicating it. It is to that class of cases that I would first call attention. It is my impression, from all that I have seen, that the manner in which this communication takes place is, in a large proportion of cases, by means of the hand, and I believe that the spaces beneath the nails, and under the skin which cover the nails, are exceedingly liable to harbour these animal destructive products; I do not say that they are limited to those parts; the skin of the fingers may also be their habitat. We know what some-

times takes place after a post-mortem examination, when it is exceedingly difficult to get rid of the smell of the corpse from the hands, cleanse them in whatever way you will. I am speaking now more particularly of what happened some years ago, before carbolic acid and other disinfectants were much known or used. It is not so difficult now as it was. At all events, I believe that the nails and the neighbourhood of the nails are the parts more particularly liable to harbour these infecting materials. It seems to me rather unlikely that the clothes carry infection so readily. Of course, where a person allows the cuffs of his coat to be dipped in putrescent material, and then makes an examination of a patient in childbirth, something may happen after that. On the whole, I think the hand is particularly efficacious in this matter. I share the opinion that some previous speakers have expressed, that very great attention should be bestowed on cleansing the hands in all cases where they are to be used in midwifery practice. A good deal has been said of late years in reference to the armamentaria that medical men should be provided with; and I think a useful addition would be a pot of carbolic ointment and a nail-brush. In the next place I would remark that it has seemed to me that any animal poison introduced from without may produce what we term puerperal pyæmia, the same as may be produced by a student who is handling surgical wounds in the hospital and attending midwifery cases. I have instances of this in the case of a medical practitioner going from one puerperal case to another. I see no difference in the attack produced in these two different ways: there is nothing whatever different in regard to the clinical features of them. Another mode in which the poison is introduced from without is due to carelessness in washing the patient or an improper method of washing. I think water ought never to be used for cleansing the perineum in the case of a recently delivered woman; I think it is far preferable to use a dry clean rag or dry cotton-wool. This plan secures greater immunity from the passage of débris from without. Then there is another method by which poison may be introduced from without—that is, in the case of laceration of the perineum; I think this mode of introduction is not very uncommon. I recollect being called into consultation to see a lady who was suffering from puerperal pyæmia. Very great care had been taken with her to keep her exceedingly quiet, and she had not been allowed to be moved from her position (lying on the back) for some days after labour. The perineum had been slightly torn. Now in this case the discharges were putrescent. It seems hardly possible to escape the conclusion that in such cases as this there is a pool of putrescent material in the vagina constantly in contact with the abraded perineal surfaces. Is it to be wondered at if some of this is absorbed? I think it is bad practice in all cases to maintain the patient too persistently in this position on the back. I think there is an advantage in turning her from side to side occasionally, in order to allow of the escape of putrescent débris from the vagina. Not that it will produce pyæmia in all cases, but if there is a laceration of the perineum it is more likely to do so. So far for the cases which come under the first category. I will now proceed to discuss those which are more difficult to define and particularise—those on which the affection does not apparently depend upon anything introduced from without. This class corresponds to the cases denominated by Dr. Barnes "autogenetic." In the first place I would remark that it is exceedingly easy to produce pyæmia in the non-puerperal woman by handling the uterus in a certain way. I have seen acute pyæmia produced by the action of a sponge tent at the os uteri, left in too long, and producing violent inflammatory symptoms, and death in a few days. It is well known that this event may happen; the uterus is so constructed that it is exceedingly easy for pyæmia to occur in it and in its tissues: the walls are very thick, the vessels are very large, they communicate very freely (I am now speaking of the uterus in the non-puerperal state), and a slight wound may at any moment set up pyæmia. I have always been unable to see any precise difference between an attack of pyæmia in a puerperal state and a non-puerperal state; the symptoms are precisely identical, they only differ in degree. I do not wish to give my remarks the semblance of a lecture, but I thought it might be interesting to bring a specimen (which I have done) representing the healthy uterus immediately after delivery, in order to show the condition of the organ at that moment. I have also brought with me a plate by a celebrated embryologist, exhibiting the condition of the uterus after delivery. Doubtless a great many teachers of midwifery are familiar with it. It will serve as a peg on which to hang the remarks to which I shall call your attention in the next place. In the uterus after delivery we have an organ precisely analogous to a sponge; it has large interstices, large spaces freely communicating, and, moreover, these spaces open into the uterus through the vessels which have communicated with the placenta, and these openings are filled up by certain clots. The natural event after labour of course is that the uterus becomes contracted, and a progressive diminution occurs in its size. This is the safeguard, and this is the important vital phenomenon which belongs to the after-period of childbirth. Now it is my belief and conviction, from all I have seen, that if we wish to obtain a clue to the manner in which puerperal pyæmia is produced, we must regard this process very attentively, and notice particularly the deviations from the normal physiological process which are so liable to occur. It is some years now since my attention was first directed to the fact, but it is a fact which I have been able to confirm by repeated observation since, and which I deem to be of the greatest importance in reference to this matter—that concurrently with the commencement of the attack of puerperal pyæmia the uterus is found to be enlarged; in other words, in a state in which its involution is absolutely retarded. I have never seen a case of puerperal pyæmia in which this condition of the uterus was absent, and absent at the very commencement of the malady. It seems to me, if this is a fact, that it is an exceedingly important element in the explanation of cases of puerperal pyæmia, of whatever kind they may be. It has seemed to me, in endeavouring to carry out explanations of these cases, that the thing which fails is the contraction of the uterus. That is where the break-down originally occurs. The woman has a large bleeding, for instance, at the time of childbirth. The want of contraction in the uterus acts in precisely the same way as if all the locks of a house were taken off, and ingress allowed to any burglar who wished to enter the premises. This is precisely what happens, in fact, after a labour; the contractile power of the uterus fails to a certain extent, the expulsion of the débris ceases, and there occurs a suction action in the uterus by which the débris are taken up into the circulation. I explain those cases in which the scarlet fever poisons and other fever poisons apparently produce disease in this way: they destroy the vitality of the patient to a certain extent, they take away the safeguard, they abolish the contraction of the uterus, and they produce a paralysis of the uterus for the time being, and having once produced this paralysis the rest of the explanation is sufficiently simple. The pyæmic process immediately passes into the uterine sinuses, and it is a question whether the patient will survive, or whether the injuries in this manner will end in speedy death.

MR. CALLENDER.—I came here to listen and to learn, but as you have called upon me I feel bound to offer some remarks on the subject under consideration; and I do so the more willingly because I quite agree with what fell from Dr. Barnes with reference to the fact that although we may be all engaged at various points at what I may call the professional circle, yet we are all attracted to one common centre, and at that centre I read the words "Prevention and cure of disease." I must be allowed to draw upon my experience as a surgeon in referring to this matter, and I feel that you will be the more indulgent to me when I recall the fact, already referred to by several speakers, that there seem to be many points of resemblance between affections such as erysipelas and septicæmia, which are constantly coming under our notice, and the disease of puerperal fever, which is at present under discussion, and which has been held by many speakers to be in many instances due in its origin to septic causes. I think that, amongst others, one speaker said that at least three-fourths of the cases were traceable to such an origin. With regard to one's experience of septicæmia I prefer to limit myself to this term, for if I venture to speak of other affections which I have heard spoken of, and which are closely allied to this disease, such as those in which the veins are implicated, I fear, after the remarks that have been made by Mr. Jonathan Hutchinson, I should be regarded as almost a heretic if I ventured to propagate the views I hold as to the part that the veins

have in these affections. I confine myself to septicæmic affections, and I say that, so far as I can judge from practical experience, we must consider them with reference to two distinctive sets of signs, those which may be spoken of as speculative, and those which may be regarded as facts. With regard to these speculative matters, I think it is purely a matter of speculation (although there seems to be a common concensus that such is the case) that these affections are due to the influence of some septic matter or poison. Then beyond this it seems to me quite a matter of speculation as to whence that poison is supposed to come. There are some who hold the idea that the poison is to be found in germs which come from without the patient; there are others who seem to hold the view that the septic poison is generated in the woman herself, as the result of the production of decomposition; and there are yet others who say that the septic material is due directly to the products of inflammation. All these I hold are mere matters of speculation, and what I should like to put before the Society is the question, how far we have a series of facts to bring to bear upon these various speculations which have been offered to us? Speaking entirely from a practical point of view, people must be allowed to speak from their own experience. There are certain points which have forced themselves on my attention—certain negative facts, such as these. I hold that in the treatment of patients in a hospital ward, the first point we have to attend to is that there must be no foulness about the wounds; the wounds must be absolutely clean. Directly we see anything unpleasant about them, we must take care to remove it. The point is that no foulness must be capable of being recognised. The wounds should be most carefully kept from any contamination from other wounds, both by the nurses and dressers; and, in regard to sponges and such like materials, all possibility of contagion from this source should be avoided. This leads me to remark that in the treatment of wounds a patient will be tolerant enough of decomposition which may be set up in a wound on his own body, but will be intolerant of poison conveyed to him or her from any other wound. That is another point of which we must be extremely careful. It is a fact that is constantly forced upon one's notice, the possibility of a wound being contaminated from another wound. Then the patients should, as far as possible, be isolated. It is easy enough to do this in a surgical ward, where a case in which there is an open wound may be surrounded by cases not suffering from wounds, so that they can be for all practical purposes isolated. Then no discharge should be allowed to accumulate in a wound, not merely the discharge resulting as a consequence of the ordinary inflammatory process, but the discharge thrown out of the wound during the first few hours after it has been inflicted. I know nothing more irritating or acrid than the fluid in a wound immediately after an operation, if it is allowed to accumulate. Then, again (and I think this is another point of great negative moment), there should be no movement of the parts; the wound should be kept absolutely at rest, allowed to heal from the beginning, and not permitted by jars and jolts to reopen so as to create new wounds, through the cracks or fissures of which putrid or other foreign matters can enter. It is by such means as these that I have learned how to banish for all practical purposes such affections as those commonly called pyæmic (though I prefer the term septicæmic) from surgical wards, except now and then when we make errors, and trouble arises in consequence. No such thing as pyæmia exists in the hospital wards under my immediate care. By the experience we get from these facts we have some light thrown upon the worst of the suspicions that I spoke of—namely, the suspicion that these cases of septicæmia are in some way connected with the presence of a poison. I think also we have some light thrown upon what is patent to us all—that which has been ably spoken of by Dr. Graily Hewitt, the sort of common sense rule that should guide us with regard to the treatment of women in a puerperal state, such as the maintenance of absolute cleanliness and absolute quietness. In addition to this we do not, from these facts, get much light thrown upon what I further spoke of as a matter of suspicion regarding the cause of the disease. I do not at all see my way clear as to the exact source of the poison. It may be in the air, it may be brought to the patient through her attendant, it may possibly lie in the festering of a wound, or be present in the direct products of inflammation; but I do not see my way to the absolute cause. All one can see from the collection of facts surrounding one's cases, extending over a considerable time, is that septicæmia is a disease which, in surgical practice, may be absolutely prevented. Here I should like to answer one of Mr. Spencer Wells's questions whether the disease can possibly be produced. I entertain myself no doubt that pyæmia and septicæmia can be produced. If after I had performed an operation upon a patient in otherwise good health I left that patient unprotected by all the many means that one is bound to adopt to ensure as a matter of certainty that a case shall be conducted towards a successful termination, it would be a mere matter of chance whether that patient did or did not suffer from pyæmia. I say a matter of chance because people differ very much among themselves; a patient may be strong and robust, and resist any poison brought into contact with the system, or a patient may be feeble, and readily succumb to the slightest dose of poison. With reference to another question, I would say before I sit down that I hold that antiseptics are of very great use to surgeons. I think they are of the very greatest possible use in surgical practice, and I take this opportunity, as it is a public one, of saying that although I may have seemed to differ very much from Mr. Lister with regard to carrying out his principles of antiseptic treatment, I am entirely at one with him as to that matter, and I fully acknowledge the great benefit he has conferred upon us by his advocacy of the antiseptic treatment. All I wish to show is that the treatment should be carried out on a more simple plan, without that elaborate attention to detail which Mr. Lister has thought necessary.

The discussion was again adjourned.

Reviews and Notices of Books.

On the Curative Effects of Baths and Waters, being a Handbook to the Spas of Europe. By Dr. JULIUS BRAUN. *Including a Chapter on the Treatment of Phthisis by* Dr. ROHDEN. An Abridged Translation, with Notes. Edited by HERMANN WEBER, M.D., F.R.C.P., &c. 8vo, pp. 658. London: Smith, Elder, and Co. 1875.

CONSIDERING the great importance of balneotherapy in the modern treatment of disease, and the frequency with which nowadays medical practitioners are asked by their patients for an opinion as to the suitability of particular baths or spas for the alleviation of their ailments, it is somewhat astonishing that so little trouble is taken by most practitioners to acquire even a fair general knowledge of the subject. No doubt much of this absence of real acquaintance with balneotherapy is due to the fact that until recently no very concise treatises upon the matter in the English language existed. Whatever was known for certain was scattered here and there through different publications. The present work, which we have read very carefully, like those of Dr. Macpherson and Sutro especially, does really supply a deficiency. It conveys an excellent idea of the curative effects, not only of the more complicated, but also of the simplest kinds of baths and waters; and it discusses the effects of these agencies in relation to other things which must always be taken into account in balneotherapeutics, such as change of scene, air, climate, diet, mode of life, and the like. The author deals with his subject in a scientific spirit, and apportions joint credit to each of these several circumstances, whilst he claims for baths and waters only such influences as can be truly said to exist after a rigorous observation of facts. Dr. Weber, in his prefatory remarks, observes that one of his motives for editing the work was the hope of inducing teachers at our medical schools regularly to deliver a short course of lectures on the subject of both waters and climate, and he adds that he does not know a work better adapted than the one under notice to form the basis of such a course. The remarks we

have already made indicate our agreement with this view of the editor.

The work may be said to be divided into five books. The first deals with the general principles of balneotherapy; the second with mineral baths, such as alkaline, saline, sea, sulphur, mud, and pine-leaf; the third with the drinking of medicinal waters; the fourth with the balneotherapy and climatotherapy of chronic pulmonary consumption; and the fifth with the methods of balneotherapeutics in actual operation at the spas, and the use of artificial mineral waters. The whole concludes with a good index.

We have not space to analyse the book in detail, but we commend it as a text-book to those who wish to acquire a good general knowledge of baths and waters, and as a book for constant reference by the practitioner in forming his decision as to the best kind of bath or spa to be recommended for his patient.

The Army Medical Service in the Past and Future. An Expostulation. By A.M.D. London: Churchill. 1875.

A Protest against "The Army Medical Service in the Past and Future." By ZERO.

Concerning the Army Medical Department. By "UNION." *Indian Railway Service Press.* 1875.

Army Medical Service. A Contrast. By "X."

Rough Notes on the Army Medical Department.

SUCH are the titles of a few of the pamphlets that have reached us on the Army Medical Service, and "the cry is, still they come." The literature on this subject will soon require a special column, or even a special journal to itself. Out of the pamphlets already published, the medical officer must be indeed fastidious and hard to please who cannot find one adapted to his taste or views. We can only pretend to indicate our opinion of their respective merits. Without doubt, Dr. Hamilton's pamphlet, as we have already said, is the best from the regimental point of view; and, apart altogether from the question of organisation, its author thoroughly deserves the gratitude of the profession and the Medical Service for the way in which he has summarised the grievances of the medical officer, and discussed his relative position to that of other officers and members of other departments. "A.M.D." puts forward a statement of his views on the Past and Future of the Service. His hopes and sympathies are with the new principle of organisation, against which, and our expostulator, "Zero" enters his protest. By far the ablest pamphlet, however, on the general system which has yet appeared is that written by "Union," and published in India. Its author brings considerable ability and practical knowledge of the Service to bear on the subject he discusses. He has compared the relative advantages and disadvantages of the two systems of medical organisation with one another after a more elaborate and exhaustive method than has hitherto been done; and we would strongly recommend those interested in the subject to read for themselves what he has to say. We do this with the consciousness that we are doubtful in regard to the correctness of some of his statements, and opposed to some of his views, as, for example, the military titles assigned by him to medical officers. "X," by the way, in his short but able production, makes a good point in speaking of rank. He says: "Medical officers possess no rank." Relative rank is an abbreviated expression for what is comprised in allowances. Argument founded on the opposite assumption is "an attempt to balance substance with vacuum." The medical officer leaves the army as he began, Dr. ——, and he comes at a mature age with an expensively procured profession, and sells it to the State for money only. The military officer, on the other hand, "purchases from the State a title which is negotiable for no mean sum in the coin of social standing." Under these circumstances it is absurd to institute a comparison between the relative positions of the two. The Marquis of Hartington said in his speech on the Regimental Exchanges Bill, that the "*prestige* of a commission was a saleable commodity." Last, but not least, is the short pamphlet of sixteen pages in which the "Rough Notes" are embodied. This is a temperate and practical statement of the case of the medical officers, and we perceive that its author has availed himself of remarks that have appeared in this journal. There is no doubt as to the fact that the majority of medical officers are in favour of the regimental, and opposed to the general system. We have, however, given our opinion on that subject, and, let the outcome be what it may, we can only hope that it will be for the good of the Service.

Manual of Family Medicine for India. By JOHN SHORTT, M.D., M.R.C.P.L., F.L.S., &c., Surgeon-Major H.M.'s Indian Army, Superintendent-General of Vaccination, Madras Presidency. Madras: Higginbotham and Co. 1875.

AN annotation that appeared in THE LANCET of Feb. 6th last on "Medical Chests and Medical Guides," appears to have aroused the authors and publishers of domestic manuals to a considerable extent, for we have received a large number of these works. The manual now before us is in many respects likely to prove a useful book, having regard to the fact that it is written exclusively for the use of Anglo-Indians. There is a chapter devoted to "bazaar" medicines, and the names of these medicines are tabulated in the Hindustani, Tamil, and Teloogoo dialects. The chapter devoted to the explanation of the action of medicines is unnecessarily diffuse, and some of its paragraphs can be hardly comprehensible to the lay reader. It seems also somewhat enthusiastic to assert that alum and lime-juice smeared round the eyelids will in one or two applications cure "ophthalmia, known as the 'country sore eyes.'" In the chapter on natural labours a great deal too much is said about "estimating the capacity of the pelvis," "the anterior lip of the womb," "the distended perineum," &c., all of which expressions would, we suppose, be unfamiliar to the sort of readers for whom the book was written, for Dr. Shortt expressly says in his introduction, "I do not intend that this work should supersede the doctor; my object being merely to point out what is best to be done in his absence." Taking the author at his word, if nearly half the contents of the book were expunged its value would be very much increased, for, as we remarked before, these manuals "should aim at excessive brevity and superlative simplicity."

The Quarterly Journal of Microscopical Science. Edited by Dr. PAYNE, Dr. LANKESTER, and Mr. DYER. April, 1875. Churchill.

THE papers contained in this part are—1. On Chlamydomyxa labyrinthuloides, a new fresh-water sarcodic organism, by Wm. Archer. 2. On the Thread-blight of Tea, by the Rev. M. J. Berkeley. 3. Observations on Spontaneous Movements of Nucleoli, by P. Kidd. 4. The Structure of the Pacinian Corpuscles considered with reference to the Homologies of the several parts composing them, by Ed. Schäfer. 5. Some Account of Modern Researches into the Nature of Yeast, by A. W. Bennett. 6. On the Invaginate Planula or Diploblastic phase of Paludina vivipara, by Dr. Lankester. 7. On the Colouring-matter of Bonellia viridis, by H. C. Sorby. This paper is followed by numerous notes and memoranda in regard to recent advances in Microscopy.

THE LANCET.

LONDON: SATURDAY, MAY 15, 1875.

IT will be seen by our "Parliamentary Intelligence" that Mr. WADDY has put his question on the present system of half-qualifications and numerous competing medical examining bodies, and that he has received a reply from Lord SANDON. The question is very opportune, and the answer is not altogether unsatisfactory. Lord SANDON gives us to understand that—though only quite recently—the attention of Government has been called to this subject, which he intelligently says is a large and complicated one. He states that, although the Government has no official information from the Medical Council on the subject, he has reason to believe that attempts are still being made to form conjoint boards for giving diplomas that shall constitute complete qualification in all branches of the medical art. He cannot say how far these efforts will be successful, but he points to the Amending Act of the Apothecaries' Society of last session and to the similar Bill now before the House respecting the College of Surgeons as proofs that the matter is not being left alone by the medical bodies.

It is gratifying to know that the subject has been brought under the notice of the Government, and Mr. WADDY must be thanked for directing the attention of the House to it. The fact is that the system is one so absurd and so unjust that it only continues because of its somewhat technical character, and of the extreme difficulty of getting the House of Commons to take any interest in such a subject. It is simply impossible to justify a system which requires a student to submit to two perfectly similar examinations in order to get a complete qualification; and it becomes more and more hard as the demands of medical education become more and more costly and exacting. But what chance is there of the House of Commons ever looking at this matter from a student's standpoint? The system has only one recommendation—that it maintains, as by a heavy taxation on those entering the profession, the various medical and surgical corporations in isolation and independence, as if surgery and medicine were perfectly distinct arts and required distinct guilds to represent them, and as if the great want of the State were not a body of well-educated general practitioners fit for every emergency. The very number of these corporations, having all to live, and living mainly by fees drawn from men passing through their portals, tends to lower the nature of the examinations by which students enter the profession; so that the system is not only unjust to those seeking to enter the profession, but to the public, as tending to keep down the standard of professional knowledge and competence. All this has not only been proved and admitted, but is obvious.

Though Lord SANDON says that attempts are still being made by the bodies themselves to effect this unification of the licensing bodies, and by so doing abolish the system of half-qualifications, he cautiously admits that he cannot say how far these attempts will succeed. We can assure Lord SANDON that the more he inquires into this matter, the more hopeless will he be of success from the spontaneous action of the bodies, either in England, Ireland, or Scotland. It is not pretended that there is a chance of success in either Scotland or Ireland. In fact, the attempts are virtually at an end in these parts of the kingdom. And our readers know that even in England the most sanguine friends of the Conjoint Scheme are in a state of despair. But, even if the Conjoint Scheme for England were completed and in action, it could never contend with the examinations of Scotland or Ireland. Lord SANDON may well say that the question is large and complicated. Let us admit, too, that the interests affected are also large and complicated. At the same time, the public is deeply concerned in the system by which the competency of its medical men is tested. The present system cannot be defended, and any Government that will reform and rectify it will do a great service to the public and the profession. If the Government is not prepared to bring in a Bill, it might well refer the question for primary consideration to a Royal Commission.

THE debate at the Pathological Society, on the Germ Theory of Disease, has come to an end; and the result of it cannot but be very gratifying to Dr. BASTIAN and those who hold with him that the theory of vital contagia is insufficient to account for facts. For it must be confessed that an impartial review of the whole discussion leads to the conviction that, so far as the arguments adduced are concerned, the balance of evidence is to be found on the side of those maintaining the physico-chemical view. To this end the singularly able and temperate speech of Dr. MURCHISON largely contributed. It was indeed a model of precise argumentation; for, dealing with matters which he has so long made a special study, and confining himself strictly to the limits of the general question, the speaker laid his conclusions before the Society in so lucid and exhaustive a manner as almost to carry conviction with them. It would, indeed, have been matter for deep regret had the premature close of the debate led to the omission of Dr. MURCHISON'S contribution. On the other hand, it will always be justly said that the question was debated too much by one side, and that it is not a fair reflex of the current of thought upon the subject. This one-sided character was given to it by the absence of such authorities as LISTER and SIMON, and the reticence of Dr. SANDERSON upon a theory which, as Dr. BASTIAN conclusively showed, he evidently favours. It must be confessed, however, that far too little weight has been assigned to the remarks of Dr. MACLAGAN, whose hypotheses were dismissed by Dr. BASTIAN in as few words as Dr. SANDERSON dealt with Dr. BEALE'S views. Even Dr. MURCHISON'S striking argument in favour of a chemical contagium—that certain inorganic substances, as arsenic, show a special proclivity for certain structures, no matter by what channel they are introduced into the body,—does not affect the counter hypothesis of Dr. MACLAGAN—viz., that the special contagious organism of a specific disease requires, parasite-like, a special nidus for its

development. Either hypothesis is valid; but the one is not destructive of the other.

Turning now to the remaining speeches, it is curious and not uninteresting to note the extreme discord between facts observed with regard to the presence of organisms in the blood and inflammatory fluids in different diseases. Much of this discord depends, doubtless, upon the exact criteria of such minute vital entities, and the difficulty experienced in discriminating them from protoplasmic masses and granules which are normal constituents of the blood and fluids. But in many cases this ground of confusion was wanting, since where the one observer had found organisms another had failed to find anything approaching to them. During the past two years this search after organisms has been pursued by Mr. WAGSTAFFE and DRS. GOODHART and MOXON; the first-named observer has met with success, the others have signally failed to find any bacteria or actually vital organisms except in the pus from open wounds and from one suppurating joint, and in inflammatory blood after being kept a short time after removal from the body. It is not a little curious that Mr. WAGSTAFFE did not find these organisms in the pus from a case of suppuration of joints, while Dr. GOODHART did, as opposed to the generality of their respective observations. Nor did Mr. WAGSTAFFE find them constantly present; but he found them in open wounds, in the blood of suppurative fever, strumous disease, and acute syphilis, in the blood and pus of pyæmia, and in a striking case of "surgical" fever following amputation, in which the rise and fall in the number of microzymes in the blood coincided with the rise and fall in the temperature of the body. This case brings further support to Dr. MURCHISON'S view that the spirilla of relapsing fever can only be explained by its being regarded as a heterogenetic product of the altered blood. Dr. PAYNE'S remarks were valuable as arguments in favour of forms of bacteria in association with putrefaction being distinct from those essential to septic processes—a distinction advocated by COHN, but controverted by LISTER, whose experiments show that the same bacterium may be modified exceedingly, according to the nature of its environment—an admission which Dr. BASTIAN was not slow to point out as favouring his theory that bacteria are only pathological products. It would, however, have been more satisfactory had Dr. BASTIAN pursued the subject more fully in the direction indicated by Dr. PAYNE. But, in fact, his reply was that of one who had no serious objections to meet; he questioned whether Mr. WAGSTAFFE'S "organisms" were organisms at all; he criticised briefly the elaborate and striking hypothesis of Dr. MACLAGAN; and he could not resist expressing his surprise that Dr. SANDERSON did not support a theory which he seems to countenance.

In conclusion, it must be confessed that the *de novo* origin of specific diseases is in many instances the only alternative, and more probable than the germ theory; but it can hardly be expected that the upholders of the latter theory will be inclined to go further than this at present, the evidence in favour of the cause and dissemination of contagious disease by means of specific germs being so strong as it is. Nor, indeed, whether the hypothesis of all-pervading germs or that of obscure and occult physico-chemical processes be the true one, it happily cannot affect the practical value of the use of antiseptics in surgery, or stay the progress of preventive medicine. One thing, indeed, the debate has shown, and that in a most striking manner: it has shown the need of further observations upon bacteria and allied organisms in relation to specific diseases. It is easy to say they are only "pathological products," or to assert that they have a causal relation to the diseased processes with which they are found, but it is evident that neither position has been fully proved; and the discrepancies among observers themselves render the need for further research most imperative. Such a research might well be initiated by a committee of the Pathological Society; for, although the labour involved in it is great, and the time over which such observations must extend before it will be wise to draw deductions from them must necessarily be long, yet there is no lack of skilled and willing observers, and the material for observation is unhappily but too abundant. If such a scheme be practicable, and the Council of the Society think fit to propose it, the result will be far more valuable than the multitude of isolated data from scattered observers now recorded, conflicting as many of these are. The valuable work carried on by Dr. SANDERSON by the direction of the Medical Officer of the Privy Council would thus receive further extension, and material would be afforded for a definite and, it may be hoped, a final solution of this vexed question. At any rate, this debate will have been of some service, if only in directing attention to the lines of research that must be diligently and carefully pursued for many years before it will be possible to formulate any theory which shall fully embody all the facts.

WE remember being once struck with an observation made to us by an uncommonly shrewd and successful man to whom we had applied for advice and assistance in getting a young friend an appointment. He seemed anxious to ascertain whether our friend in question had ever passed a competitive examination; for in that case, he declared, he would have nothing to do with him. On being pressed for his reasons for this determination, he said something to the following effect:—That he wanted to reap some benefit from the abilities of anyone in his service, and that he could not expect much from one whose energies had been already severely taxed, and probably exhausted in "cramming" for a competitive examination; besides, he contended, if a young man had such great natural capacity and receptive faculties as still to have a good deal in him after having occupied a very high position in competition with others, "the chances are that he will have become very conceited or a literary prig." We do not for a moment say that this is the whole truth; but that it contains an element, and possibly a large element, of truth is, we think, very probable. The brain of a young man of average abilities may be likened to a soil capable of producing, under ordinary circumstances, an average crop; but which requires to be highly cultivated and stimulated in order to produce a larger crop, after which it probably becomes exhausted. There is a good deal to be said, unquestionably, for competitive examinations on the score that they open the

door to a number of men who possess culture and ability, but do not possess any interest to push their fortunes; and such a system is far preferable to the exercise, and often abuse, of the old plan founded on patronage. But we feel confident that the public services do not always obtain the best material by competitive examinations, and that injury, physical and mental, is often done to the candidates themselves. The great difficulty is to find a good substitute. The system, which has been abolished in the navy, is said to have proved unsatisfactory for the Indian Civil Service; but it holds its own for the Royal Military Academy, Woolwich, and the army generally. The Scientific Corps of the army, being very popular services, always attract a large number of excellent candidates; and, as mathematics holds so prominent a place in the examination, the competitive test is probably a good one. Plausible and satisfactory as this method of testing may appear, however, it inevitably leads to a system of cramming and a plentiful crop of crammers; and the battle is between the examiners and the crammers. The former insensibly adopt a certain line, follow a certain track, or repeat themselves; and the latter are not long in taking their soundings. The result is that you may hear boys congratulating or sympathising with one another on their good or ill luck as their crammers have been more or less successful in indicating the character of, or hitting off the questions at, these examinations. It is chiefly in the case of young boys, however, that this premature forcing the intellectual faculties and cramming the memory must prove injurious. If anyone will be at the trouble of perusing the Report of the Committee to inquire into the system of training naval cadets, with the Minutes of evidence, we think he must recognise the evils attending the competitive system for mere lads. It certainly seems absurd that a boy's career in life should be stopped at the early age of twelve or thirteen in consequence of his failure—for it need not be his inability—to compete successfully in a severe examination. The character of a boy at that age is not formed; and a really able lad may be idle, thoughtless, or nervous, or he may fail from a temporary headache or a trifling indisposition from overeating, and find himself debarred from entering on a profession for which he is eminently qualified by his tastes, character, and the traditions of his family. Imagine unfortunate youths of this tender age being subjected to a process of "grinding or cramming" for ten or eleven hours daily by way of preparation for an examination! It is not a process of training, but one of straining, injurious to body and mind alike.

Dr. SCHMITZ expressed, in his evidence before the Committee, his strong belief that in England we are on the high road to destroying the mental powers of young people by this strain put upon them at an early age, which seemed to him to be calculated to produce disastrous effects afterwards. Dr. SCHMITZ mentioned that some years ago the Russian Government had asked his opinion of a plan which they meant to introduce into that country, and which was contained in a voluminous book. He wrote briefly and strongly that if the Russian Government carried out their system, the result would simply be in a short time to rear a generation of imbeciles; and he added that he knew many instances of young men in England who had broken down completely from anxiety in preparing for these examinations. It is the fashion at the present time to reverence everything German, because it is the fashion to worship success; and the fact may therefore have some importance in this country that in Prussia and Germany generally they are particularly careful not to overburden the minds of young men; they have even gone so far in the last two years as to close the schools entirely in the afternoon during the hot weather in summer. It is not so much the mental work to which young men are subjected in preparing for competitive examination—although this often curtails the necessary amount of out-door exercise,—but it is the worry and anxiety that prove detrimental to the health. Some young fellows are never free from this fret and anxiety. Their success or failure at an examination is their thought by day and their dream by night; and they often pursue unconsciously in their sleep and talk aloud in their dreams of those subjects which have exhausted their brains during the day. What is this but the result of an irritated and ill-nourished brain? Their bodily functions are robbed of that nervous stimulus which they require for their normal and healthy play; and in the end it may happen that the State gets in its servants selected by successful competitive examinations the rinsings of an exhausted brain, instead of the products of a vigorous and healthy one, disciplined and strengthened by a well-adjusted course of training.

THE splendid meeting at the Mansion House on the 7th inst. will not only restore the Metropolitan Hospital Sunday to the place in the confidence and interest of the public which it had prior to the unfortunate meeting of the 8th of March, but will tend to give this institution a higher and securer position than it has ever yet had. Nothing could well have been more significant or influential. The meeting was composed of clerical and lay representatives of all the churches which have contributed to the fund, and of the Council. Clearly this is the true constituency of the fund, which has the right to dictate the way in which it shall be administered and distributed. Not only was every leading denomination represented, but it was represented by its leading men—such as the Bishop of LONDON, Cardinal MANNING, the Rev. Mr. PERKS, Dr. KENNEDY, Professor MARKS, &c. Parliament, too, was well represented. Nothing could be more surprising than the confidence displayed by this meeting in the Council and in the Committee of Distribution, or its disapproval of the meeting of the 8th of March. The attempt to pass a resolution excluding those connected in any way with hospitals from the Committee of Distribution was a complete failure. Sir SYDNEY WATERLOW was specifically excepted. The absurdity and unpopularity of excluding him had become too apparent for it to be insisted on at this meeting. The resolution seemed to be aimed at the medical members of the Distribution Committee, Dr. BENNETT and Mr. DURHAM. Only two or three hands were held up in its favour. We rejoice at this result. These gentlemen labour under the disadvantage that they are connected with hospitals so old and wealthy as to make it difficult for them to sympathise with poorer and younger institutions. But this connexion also makes them independent. We may trust these gentle-

men to perceive that public opinion is in favour of giving fair consideration to all kinds of institutions that can show anything like good work or a *raison d'être*. If they exhibit any narrowness or partiality, they can be removed and replaced by others. It would be strange indeed if the Distribution Committee should not do its work better by having two medical men of standing on it; and it would have been a high discredit to the profession if the recent opposition to medical men honourably connected with hospitals—emanating, we are sorry to say, from a medical quarter—had succeeded.

The most important points determined by the meeting of the 7th of May are: that the constituency of the fund is to consist of "such congregations as have forwarded contributions to the fund during either of the two preceding years," or rather of the minister and two laymen representing such congregations. This constituency is to elect the Council, which is to consist of fifty clerical and fifty lay members, which shall have power to arrange for the collection, to appoint the Committee of Distribution, to receive its report, and frame such rules as may be needful for the proper administration of the fund. The question of payments made by or on behalf of patients is left to the discretion of the Distribution Committee. Resolution 5 determines the way in which the basis of award is to be arrived at. It runs as follows:—

"That awards to hospitals &c. be primarily based on the total expenditure of each institution after deducting therefrom (1) the income derived from endowments and realised property; (2) the amount received in legacies exceeding £100 each; (3) the amount of expenses of management. But that in every case the *merits* and pecuniary *needs* of the institution concerned be fully inquired into and considered by the Distribution Committee; and that the award made be determined in accordance with the judgment of the Distribution Committee upon such merits and needs, provided that in no case shall the grant be so reduced or withheld until a conference shall have been sought with the Managing Committee of the said hospital, &c."

This resolution greatly enhances at once the duty and responsibility of the Distribution Committee. It confers a power of inquiry which might be used so as to make the Committee appear inquisitorial, and it appeals largely to the discretion of the Committee. After the ample and, withal, beneficial discussions of the last two years, the Committee must know well the general feeling of the public with regard to the different kinds of institutions, and will pretty certainly act accordingly. We congratulate Sir SYDNEY WATERLOW and the Council generally on the new expression of confidence in them implied in such a meeting as that of the 7th of May carrying every one of the resolutions suggested by the Council for its acceptance; and we wish the Metropolitan Hospital Sunday all the success which seems, indeed, ensured by such hearty co-operation of men of all creeds as characterised that meeting.

IN London 1489 deaths were registered last week, including 1 from small-pox, 25 from measles, 35 from scarlet fever, 9 from diphtheria, 71 from whooping-cough, 29 from different forms of fever, 11 from diarrhœa, and 354 referred to diseases of the respiratory organs. Two deaths from puerperal fever occurred at Plumstead.

Annotations.

"Ne quid nimis."

VIVISECTION.

WE venture to think that we cannot be accused of having discussed the subject of vivisection in a petty spirit of class prejudice. From the moment that it threatened to occupy public attention, and before it had assumed the prominence that it has since attained, we expressed our entire willingness, and even desire, to have the subject investigated by a properly constituted Commission, with the view of determining whether the limits of legitimate investigation and experiment could not be defined. We warned our readers that it would soon get mixed up with much that was the result of ignorant exaggeration, or with irrelevant, insincere, and sensational statements. We fully recognised that animals had their rights every bit as much as a man has his, and that our conduct must be regulated by a sense of responsibility and by the broad principles of humanity and duty. We pointed out, however, that charges of cruelty come with a very bad grace from those who did not hesitate to indulge in sports that were undeniably cruel, with no other object in view than their own selfish gratification; and we said that the sum of physical suffering from this cause, and from the everyday facts connected with our supplies of food and clothing, far transcends that resulting from the practices of physiological experimenters. We are not so illogical as to hold that two wrongs make one right, but we do contend that it savours much of cant and insincerity to ignore these things as if they had no existence, while vivisectors are held up to execration. That there are circumstances under which experiments, involving the lives and suffering of animals, are justifiable, most people would be willing to allow; although they may concur with us in condemning the wholesale cruelty that has been alleged against a continental experimenter. Where, then, is the line to be drawn? Is the matter to be relegated to a criminal court to be settled by a magistrate? Is it to be fought out in Parliament after discussions maintained by those who are very likely, from ignorance of the elementary principles of physiology, to mistake appearances for realities? Instead of introducing rival Bills for legislating on this subject, without any due inquiry into the nature and extent of the evil, it would have been far better to have adopted the course we advocated from the very commencement—viz., a preliminary inquiry by a properly constituted Commission.

The question is a much larger and more difficult one than is generally imagined. Sir Henry Thompson has lately called attention to some causes of physical suffering to the lower animals quite outside and apart from the practices of physiological and anatomical experimenters, which cannot be ignored as if they had no existence; and one at least of the Bills already presented to Parliament shows the incompetency of its framers to appreciate the bearings of the subject. The fact is, that a question of this kind is not, and cannot be, circumscribed within the limits of the term vivisection. Some deliberate expression of opinion is, at any rate, called for on such subjects as aristocratic pigeon-matches and stag-hunting, as followed in this country, for example, which seem to us cruel and reprehensible sports, without anything to justify them beyond the fact that they happen to be fashionable, and serve to minister to the selfish gratification of those who participate in them.

Since the above was in type we have received a copy of the Bill prepared and brought in by Dr. Lyon Playfair, Mr. Spencer Walpole, and Mr. Evelyn Ashley. As far as can

be gathered by a somewhat hurried perusal of this document, it appears to us to be a fair, just, and well-considered measure, and incomparably superior to another Bill in the Lords.

PUBLIC ANALYSTS.

WE fear the confidence of the public in analysts and chemical experts must have been very much shaken of late. It seems now a matter of course that whenever a case under the Adulteration Act is taken into court, the most startling discrepancy should be discovered to exist between the results of the analyses, made respectively on behalf of the law and on behalf of the defending trader. These lamentable exhibitions of conflicting professional testimony have hitherto been confined to the metropolis. The following case, which occurred at Stoke-upon-Trent, although not so bad as many we have felt it our duty to comment upon, is not of a nature to inspire blind trust in the skill of analytical chemists.

In proceedings a few days since before the stipendiary magistrate at Stoke-upon-Trent it appeared that half a gallon of ale was purchased of a publican, and a sample was forwarded to the county analyst, who certified that it contained 95 grains of salt per gallon (we may parenthetically observe that the addition of 50 grains per gallon is permitted). Some of the ale was then sent to Dr. Dupré, another public analyst, and he certified that it was adulterated with 136 grains of salt. Meantime another half-gallon of ale was drawn by the publican in the presence of the inspector, who forwarded it to Dr. Hill, the borough analyst of Birmingham, who stated that it contained only 9·73 grains of salt per gallon, and was perfectly pure ale. A portion of this sample was also sent to Dr. Dupré, who certified that it contained 48 grains per gallon. The defendant was fined £20 and costs, but notice of appeal was given.

Our contention is that a central analytical authority should be established in London, to whom all disputed cases should be referred for final and absolute decision. It would be necessary to engage gentlemen of eminence for this responsible court of appeal, and they should be well paid by the State, and restricted from the performance of any other duties. In such a way a thoroughly independent authority would be secured in whom the country would place confidence.

Since writing the foregoing the question has been brought directly before the House of Commons. It may be remembered that Clause 21 of the Adulteration Bill gives power to the justices to have articles of food and drugs tested by the analyst of another district. Mr. Sclater-Booth, in answer to a question, said he was prepared to accept an amendment to the effect that the justices might cause the articles to be analysed by persons appointed by the Commissioners of Inland Revenue, who should, after examination, furnish the justices with certificates. After a desultory conversation, in which Mr. Muntz, Sir H. Peek, Dr. Cameron, Dr. Playfair, and others took part, the following amendment was agreed to:—That the justices might send the articles to be analysed "by chemical officers in the employment of the Inland Revenue, who shall thereupon make the analysis and give a certificate to such justices of the result of the analysis, and that the expense of such analysis shall be paid by the complainant or defendant, as the justices may direct."

We venture to prophesy that the Inland Revenue officers will have plenty of work to do, and that an opportunity will exist for the foundation of a School of Analytical Chemistry, which ought to attract to and send from it thoroughly good men.

CIRRHOSIS OF THE LIVER IN CHILDREN.

EXAMPLES of cirrhosis of the liver in early life, apart from congenital syphilis, are certainly of infrequent occurrence; but that they are sometimes met with is sufficient ground for the necessity of extending the somewhat narrow bounds of the etiology of this affection. Two cases recently recorded by Dr. J. Cazalis (*Le Progrès Méd.*, No. 12, 1875) draw attention to this subject, and make it probable that exposure and bad living alone are sufficient to bring about the disease. The first case was that of a girl seven years of age, who had suffered all the privations of the siege in Paris, and who died after two months' illness brought on by those privations; the symptoms being chiefly ascites, anasarca of the lower extremities, hydrothorax, and pulmonary œdema; there was also slight albuminuria. Paracentesis abdominis was performed a fortnight before death, and was followed by subacute peritonitis, to which the little patient succumbed. At the autopsy the liver was contracted, granular on the surface, and presented all the characters of cirrhosis. The spleen was much enlarged. The second case was also of a girl aged nine years, who had lived for many years in a damp *rez-de-chaussée*, and whose health had been impaired for some time. During three years before admission she had frequently suffered from epistaxis. Swelling of the abdomen, followed by swelling of lower limbs, had gradually taken place, but had been greatly and rather suddenly increased under a febrile attack which occurred a few weeks before admission. There was slight jaundice, extreme ascites, for which paracentesis was performed, but no albuminuria. Death occurred from peritonitis, and at the autopsy, in addition to evidence of recent, there were also signs of chronic, peritonitis in the splenic and hepatic regions. The liver was no larger than the adult fist, was very granular on the surface, very tough, and traversed throughout by interstitial fibro-nuclear overgrowth richly supplied with vessels. The new material encroached upon the lobules, the marginal cells of which were atrophied, the central being fairly well formed, but loaded with fat and pigment. The spleen was greatly enlarged, exceeding the liver in size. Dr. Cazalis remarks that in neither case was there any syphilitic history in the parents, nor were the anatomical characters of the liver those generally described as due to congenital syphilis. Direct alcoholism was put aside from the youth of the patients; nor were the parents intemperate. Malarial poisoning and cardiac disease, the two remaining causes known of cirrhosis, could also be put aside. He thinks, however, that the exposure and privations endured by the one, and the life of the other in the damp and unhealthy *rez-de-chaussée*, might possibly be the cause of the development of cirrhosis in these two children.

THE COST OF A HARD WINTER.

WITH the exception of a few days at the end of December, the weather during the winter of 1874–5 was not intensely cold, and yet the winter was both long and hard. It contained two long periods of low temperature—the last six weeks of 1874 and the two months of February and March,—during the latter of which the effect of the cold was intensified by northerly and easterly winds. The month of January, which intervened between these two cold periods, was remarkably warm, and probably helped to make the cold of February and March still more fatal. We recently alluded to the effect of the winter's cold upon the London population; the publication of the Registrar-General's Quarterly Return relating to the first three months of this year enables us to calculate pretty accurately the total death-toll which was levied in England and Wales during

the past hard winter. The average annual English death-rate in the first quarter of the thirty-six years 1838–74 was 24·8 per 1000, and in the corresponding period of the three warm winters 1872–3–4 it did not exceed 23·3. In the first three months of this year it was equal to 27·5 per 1000. Taking into account those who were killed by the cold and whose deaths were registered in the last quarter of 1874, the number of persons who succumbed to the severity of last winter in England and Wales cannot be estimated as under 35,000, of whom 15,000, says the Registrar-General, were persons aged sixty years and upwards. This is the number actually slain; the wounded—those whose constitutions have been impaired, if not shattered, by severe attacks of bronchitis, pneumonia, pleurisy, or asthma—cannot be counted. As nearly half the number of those who died from the effects of last winter's cold were aged over sixty years, whereas the proportion of persons of this age in the English population does not exceed 7·4 per cent., it is evident that elderly people are the greatest sufferers. The Registrar-General on this point says:—"Compared with the deaths recorded in the mild first quarter of 1872, those in the first three months of this year showed an increase of 20 per cent.; the increase in the deaths of persons under sixty years of age was but 12 per cent., whereas those of persons aged upwards of sixty years had increased 47 per cent." There is one other point in connexion with the fatality from cold during the past winter to which the Registrar-General directs attention, and which is well worthy of consideration and investigation; this is the far greater excess of mortality in rural than in urban populations. The death-rate during the first three months of this year was 28·9 in the urban districts comprising the chief towns, and 25·7 in the rural districts; compared, however, with the mean rates in the first quarters of the ten years 1865–74, the increase or excess was equal to 8·6 per cent. in the urban and 16·8 per cent. in the rural population. One source of explanation of this remarkable difference is to be found in the varying proportions of persons aged upwards of sixty years in different parts of England. This proportion ranges from 5·8 per cent. in the principally urban population of Cheshire and Lancashire, to 9·9 per cent. in the agricultural population of the south-western counties. The Registrar-General adds:—"How far this result is due to the greater proportion of elderly persons in rural populations, and how much to the low wages and to the dearness of firing from which the agricultural populations suffer, can only be ascertained after careful investigation." Seasonable weather used to be considered a subject for general congratulation; more accurate knowledge on the subject, however, brings us face to face with such hard facts as, for instance, that the fourteen weeks' cold weather of last winter killed at the rate of about 350 persons a day in England and Wales. Seasonably hot summer weather in July and August is scarcely less fatal.

THE THRUSTON SPEECH.

THE Thruston Speech was delivered last Tuesday, May 11th, in the college chapel of Caius College, Cambridge, by Dr. C. H. Ralfe, late scholar of the college, and physician to the Seamen's Hospital, on the occasion of the commemoration of Dr. Wendy, a physician who practised at Cambridge during the latter part of the sixteenth century, and was a liberal benefactor of the college. Dr. Ralfe chose for his subject the sanitary and social improvements that have taken place during the last three centuries, pointing out the advance made in agriculture, engineering, and in medical science, and the part respectively played by each in adding to the health and happiness of the community. Contrasting the recent epidemics at Over Darwen and Lewes and the cholera outbreaks of 1831 and 1848 with the terrible pestilences of the sixteenth century, Dr. Ralfe said there could be no doubt that epidemic diseases were much more manageable now than formerly. This he attributed to our improved knowledge as to the mode in which these diseases spread and were communicated. It was not, at present, so much want of knowledge as the difficulty of applying our knowledge that permitted outbreaks of such diseases as cholera and typhoid to occur; for were sanitary legislation adequate to enforce the recommendations made by those who have made sanitary science a special study, he thought the recurrence of those diseases, certainly in an epidemic form, would be rendered impossible. Neither our health nor our lives ought to be entrusted any longer to so proverbially selfish and ignorant a body of men as are the local sanitary boards, as at present constituted, but a powerful central authority should be formed who would fully and completely deal with the sanitary requirements of the age Speaking of Dr. Caius, the founder of the college, Dr. Ralfe alluded to the fact that no portrait of that eminent physician was to be found within the walls of the Royal College of Physicians, although Dr. Caius had done so much for medical science, and for the Royal College of Physicians in particular. Dr. Ralfe observed that an opportunity now occurred for remedying this neglect. It was proposed to ornament the portico in front of the College buildings in Pall-mall with the statues of Linacre, Harvey, and Sydenham. Might not Caius have a place beside these worthies? Surely the founder of a college which has given to medicine such men as Glisson, Harvey, Scudamore, George Budd, Arthur Farre, George Paget, Charles John Hare, and the present distinguished president of the Royal College of Physicians, Sir George Burrows, should not be permitted to suffer such neglect. Dr. Ralfe suggested that members of Caius College might consider this matter as a personal duty, and he hoped that ere long a suitable memorial might be placed in the Royal College of Physicians worthy of the name of Caius.

THE ACCURACY OF CLINICAL INSTRUMENTS.

THE first requisite for correct diagnosis, for sure treatment as well as for scientific progress, is correct observation. But for correct observation accuracy of the means by which the observations are made is indispensable. The progress of modern medicine is in great measure due to the fact that instrumental measurement has been, as far as possible, substituted for and added to the uncertain estimate by the unaided senses, which was, for so long, all the practitioner had to rely on for ascertaining the presence of the various departures from the standard of health, and for measuring their amount. But, to possess any real value, at least approximate accuracy of the instruments employed is essential. It is to be feared that too often the accuracy which the instruments possess is not even approximate. Some, as the thermometer, are of the most delicate description and fragile in the extreme, and can only be obtained of real accuracy at a price which is a very serious item in the expenditure of a hard-worked and badly-paid practitioner. But what is worse is the uncertainty which exists as to the accuracy of those which he does obtain. He has not the time, skill, or means for testing them himself. To have them tested at the Observatory at Kew is a very troublesome and tedious procedure, for which some weeks are needed. He is, therefore, entirely at the mercy of the instrument-maker, who, with every desire to supply a precise instrument, is often unable to do so, except at a price which, compared with other less accurate makers, is almost prohibitory. A real service would be conferred on the profession if one of our societies would establish the means of testing the

accuracy of instruments. Arrangements might be made so that any practitioner bringing a clinical instrument might obtain, for a moderate fee, without unnecessary loss of time, a trustworthy statement of its accuracy. Such a work would fall perfectly within the objects for which the Clinical Society was founded, and the Society would confer a greater boon on the profession than it can perhaps in any other way if it would undertake the work.

As an example, which will probably come home to every practitioner, of the need for such an arrangement we may cite the common urinometer, an instrument in daily use by all, the information supplied by which is an important aid to the diagnosis, prognosis, and treatment of a large class of cases. It is probably not too much to say that three-fourths of the urinometers now in use are so inaccurate as to be quite untrustworthy. We recently tested, by means of standardised solutions, two urinometers, each obtained from one of our best makers. The following were the results:—One urinometer in a solution of specific gravity 1010 registered 1004; at 1020 it stood at 1018; at 1030 and 1040 it was accurate; at 1050 it registered 1057. The other instrument at 1010 registered 1008; at 1020, 1016; at 1030, 1025; at 1040, 1034; at 1050, 1043; and at 1060, 1053. Instances in which grave errors might have resulted from the erroneous readings will suggest themselves at once to those who are in the habit of using the instrument; and most urinometers possess even less accuracy than these. The examination and consideration of instruments have been left too much by our societies to anyone who chose to take up the subject, whose opinion at the best possessed little weight with the profession, and was often reduced in value by obvious crotchets or incompetence. The determination of the accuracy of instruments, and of the way in which such accuracy can be best secured, are subjects which could be taken up with great advantage by a society, the position of which gives it authority, and which numbers among its members many with special knowledge on the points involved.

ARMY MEDICAL OFFICERS.

MR. GATHORNE HARDY'S answer to Mr. Gibson's question the other night has caused some surprise, if we may judge from the communications that have reached us. Medical officers attached to regiments for five years are now given to understand, on the highest authority, that they are regarded as *bonâ-fide* officers of the regiment, and that they are consequently liable to the same mess contributions and subscriptions as any other officer. We trust that when the Minister for War announces his intentions in regard to the medical officers he will state publicly what is the organisation of the Medical Service, for, as far as we can understand, one of the main sources of complaint is, that no one knows whether the service to which he belongs is general or departmental, or a mixed one. What with Dr. Lush's announcement that he intends, immediately after the recess, to call attention to the position and emoluments of army medical officers, and with several questions yet outstanding, Mr. Gathorne Hardy's intentions, one would think, cannot be much longer delayed.

THE PROPOSED CHILDREN'S HOSPITAL.

As it appears that some objection, if not opposition, is likely to be raised to the third site which the Duke of Westminster has offered for this hospital, on the score of the objectionable nature of its aquatic surroundings, it may be well to remind our readers and others interested in this question that the Corporation, as the sanitary authority of the port of London, are bound to use all legal means to keep, not only the vessels and their inhabitants, but the "waters" within their jurisdiction in good hygienic order. The jurisdiction of the city authorities as sanitary authority of the port extends on the Thames from Teddington-lock to the North Foreland, up to high water mark, and includes also all docks, creeks, inlets, and other channels into which the tide flows. The small basins (for they cannot be dignified by the name of docks) opening into or about the site referred to ought, in common with all other inlets of the same kind, to be visited systematically by the officers of the port authority; and we may assert confidently that if these officers do their duty no objection to this site on the score of the existence of these small basins can be fairly maintained.

THE SCARLET FEVER EPIDEMIC.

THE recently published Quarterly Return of the Registrar-General tells us that rather more than 5000 persons died from scarlet fever in the first three months of this year in England and Wales, against 6081 and 8562 respectively in the last two quarters of 1874. The scarlatinal wave is evidently receding, though somewhat slowly. The annual death-rate from this disease was equal to 8·6 per 10,000 persons living in England during last quarter, and nearly corresponded with the average rate during the twenty years 1850-69. The disease showed the largest proportional fatality last quarter in Nottinghamshire, Derbyshire, Lancashire, Yorkshire, Durham, Cumberland, and South Wales. The scarlet fever death-rate was equal to 20 per 10,000 in the West Riding of Yorkshire and Cumberland, and to 21 in the East Riding. Among the many places that have recently suffered severely from the disease may be mentioned Grays (Essex), Tiverton, Yeovil, Burton-on-Trent, Louth, Great Grimsby, Frodingham (Lincolnshire), Derby, Chesterfield, Farnworth, Accrington, Stockton, Hartlepool, Aberdare, Ystradgunlais, and Carnarvon. Even an epidemic, however, in the present day has a distinctly beneficial influence upon sanitary progress. The scarlet fever outbreak has undoubtedly found out, and thus called attention to, many localities which have long been suffering from sanitary shortcomings. Take New Frodingham, in Lincolnshire, for example. This is a new neighbourhood, which owes its growth to ironworks; 9 deaths from scarlet fever occurred last quarter among its few hundreds of inhabitants, and the local registrar reports "there is great overcrowding, and the water is impure."

DIAGNOSIS OF BLOOD-STAINS.

DR. JOS. G. RICHARDSON, in an article published in the *American Journal of the Medical Sciences* for 1874, stated that in his opinion we are able, by the aid of high powers of the microscope, and under favourable circumstances, to distinguish human blood-stains from those of the pig, ox, red deer, cat, horse, sheep, and goat, with certainty, all these being smaller than those of the ordinary human red disc. This conclusion has recently been called in question by Dr. J. J. Woodward, who maintains that we can never truthfully affirm on the strength of microscopical investigation that a given stain is positively composed of human blood. Dr. Richardson, in the *American Journal* for April, in reply, somewhat modifies his original statements, admitting that great difficulty would arise in any given case; but, on the other hand, so far supports his former view that he thinks it often happens in practice that *evidence other than microscopical* narrows down the conditions of a case to the question—Is this slain human blood, or that of an ox, pig, or sheep. The microscopist can then, in such cases, from fair specimens of blood-spots as ordinarily produced, affirm truthfully that a "given stain is positively composed of human blood," should it really be so, and this he thinks he

can conclusively prove. We should not ourselves like to go beyond "a strong presumption" in this direction, and should be sorry to see this exceeded by a medical witness, however skilled in the use of the microscope, in a criminal case.

INCREASED FATALITY OF PUERPERAL FEVER IN LONDON.

THE Registrar-General, in his last Weekly Return, calls attention to a recent increase in the deaths referred to puerperal fever in London. The fatal cases of this disease registered within the metropolis in 1871 were but 182; they increased to 251, 306, and 456 in the three following years, 1872–73–74. In the first thirteen weeks of this year 82 deaths were referred to this disease, which, although exceeding by 14 the corrected average number in the corresponding period of the last ten years, were 41 less than in the first quarter of 1874. During the past five weeks of the current quarter 32 fatal cases of puerperal fever have been recorded in London, of which 10 occurred in the week ending May 8th, including 5 in South London. At a time like the present, when the disease has appeared in an epidemic form in many localities, it is certainly desirable that the deaths referred thereto should be closely watched, and the utmost caution adopted to prevent the contagion from spreading.

PLAGUE IN MESOPOTAMIA.

THE *Levant Herald* announces the appearance of plague in the district of Diwanieh, on the Lower Euphrates. The disease, it would appear, is scattered over a wide extent of country. It will be remembered that this district was the seat of a serious outbreak of plague last year. From the Hindieh Canal, near Hillah, to Samava, both banks of the Euphrates are occupied with a series of vast marshes, the islets of which are inhabited by Arab tribes. In 1867 the Hindieh marshes, after many years' absence of plague from the district, became the scene of an outbreak of the disease. Early last year the marshes on the opposite (the east) bank of the river, occupied by the Affij Arabs, were the locality of an outbreak, which is believed to have carried off 4000 people. The outbreak now reported, so far as we are able to make out from the details which have come to hand, has occurred partly in the district affected last year and partly in localities south of this district and south of Diwanieh, unaffected then. This year's reappearance of plague, taken in conjunction with the three outbreaks which happened last year—namely, one in Mesopotamia, one in Western Arabia, and a third in North Africa (Bengazi district),—suggests that plague may be about to become once more a familiar disease in the East.

INVALID CONVICTS AT PORTLAND.

AT a recent inquest held at Portland Convict Prison on the death of a convict from consumption, while undergoing a sentence of twenty years' penal servitude, the jury in their verdict made grave charges against the authorities of the prison, and especially against the assistant-surgeon. They found that the deceased did not receive proper treatment, and were unanimous in the opinion that the Home Secretary should direct an official special inquiry into the circumstances of the case, and that the general treatment of the invalid convicts in the Portland prison requires investigation. Such charges are frequently made, and so seldom substantiated that we may confidently anticipate that the assistant-surgeon and the other officials will have a complete answer to the charges when the inquiry takes place. The principal witness at the inquest was William Roupell, of Lambeth notoriety, now described as nurse ordinary in the prison.

THE MEDICAL COUNCIL.

As noticed on a former occasion, the annual session of the Council will take place on or about the 16th proximo.

The following re-elections have occurred since the meeting of the Council in July last:—University of London, Dr. Storrar; Apothecaries' Hall, Dublin, Dr. Leet; King and Queen's College of Physicians in Ireland, Dr. Smith; Cambridge University, Professor Humphry; Queen's University in Ireland, Sir Dominic Corrigan, Bart., M.D.; Royal College of Surgeons in Ireland, Dr. Macnamara; Universities of Glasgow and St. Andrews, Professor Allen Thomson.

The only new appointment since the last session of the Council has been that of Professor George Rolleston, who now represents the University of Oxford, in the place of Dr. Acland, elected President 18th July, 1874.

BIRMINGHAM HOSPITAL SATURDAY COLLECTION AND ITS DISTRIBUTION.

AT a meeting of the Committee for conducting the Hospital Saturday collection, held in Birmingham a few days ago, Mr. J. S. Wright in the chair, it was announced that the total proceeds of the recent collection in the factories and public-houses of the town amounted to £3838, and the net balance for distribution to £3500. Mr. Beattie, the treasurer, then distributed the money to the representatives of the various local institutions, who were then in attendance, according to their pre-arranged proportions, as follows:—General Hospital, £1263 17s.; Queen's Hospital, £922 12s.; Eye Hospital, £210 7s.; Children's Hospital, £277 11s.; General Dispensary, £349 6s.; Lying-in Charity, £60 4s.; Ear and Throat Infirmary, £13 13s.; Sanatorium, £166 5s.; Homœopathic Hospital, £107 16s.; Women's Hospital, £79 16s.; Orthopædic Hospital, £45 10s.; Dental Dispensary, £3 3s.

The moderateness of the expenses compares favourably with the case of the London Hospital Saturday.

THE PUBLIC HEALTH BILL, 1875.

THE Society of Medical Officers of Health have prepared and submitted to the President of the Local Government Board a series of carefully considered verbal amendments of the Public Health Bill, 1875, and supplemented these by a series of important suggestions as to the amendment of the law with respect to sewerage and drainage, water-supply, nuisances, infectious diseases and hospitals, &c., when further sanitary legislation is proposed. The Society recognises the advantages of the proposed consolidation of sanitary laws as a preliminary measure to their satisfactory reform.

LECTURES ON STATE MEDICINE.

WE would call attention to a course of lectures in connexion with State Medicine, which Surgeon-Major Dr. De Chaumont, Assistant Professor of Hygiene at the Army Medical School, Netley, is about to deliver before the Society of Apothecaries at their Hall in Blackfriars. The course consists of six lectures, and according to the syllabus will embrace a number of topics of importance to health officers and others. Their interest is not likely to be diminished by the fact that they concern many matters of daily application, and others of increasing difficulty and importance in the age and state of society in which we are living.

THE HAMPSTEAD HOSPITAL.

THE Metropolitan Asylums Board, at its meeting on the 8th inst., confirmed, by 38 votes to 5, the report of the Committee, of which an account was given in last week's LANCET. The Board adheres therefore to its conclusion to

retain the Hampstead site, offering such consolation to the residents of that village as may be found in a promise that the hospital shall only be used under circumstances of exigency. It remains now to be seen what course the Hampstead Committee of Residents will take. Pending its action discussion would be idle.

A GRIEVANCE similar to that of which the residents of Hampstead have been so long and so loudly complaining is just now agitating the minds of some of the inhabitants of West Derby. It appears that the guardians of the township have carried out an intention which they had expressed of utilising Starfield House, West Derby-road, as a small-pox hospital. This step has excited great discontent amongst the residents in the neighbourhood, and a meeting was called last week to consider the question. Dr. Carter, medical officer of health for the township, said that cases of small-pox were increasing in number in Liverpool, and, with others at the meeting, expressed the opinion that Starfield House was an unsuitable building for a small-pox hospital. The chairman of the board of guardians endeavoured to justify the action of the board in the matter, but a resolution adverse thereto was ultimately carried almost unanimously.

A REPORT of the *Hamadryad* Seamen's Hospital, Cardiff, just issued, shows that 460 in-patients and 2846 out-patients were treated in the ship during the year 1874. The committee are constrained to call attention to the large number of British ships whose owners or agents refuse to contribute to the funds of the hospital. In the month of December last there were no less than 113 refusals on the part of ships belonging to ports in the United Kingdom, while only two foreign ships refused assistance. This disclosure is not very creditable to British shipowners, whose wealth is so vastly in excess of that of similar classes in foreign nations. The latter would appear to be wisely solicitous of the health and welfare of their seamen, as we are told they very rarely refuse contributions in support of hospital-ships.

THE twenty-third annual meeting of the governors of the Hospital for Sick Children, Great Ormond-street, was held recently, under the presidency of the Hon. A. Kinnaird, M.P. The report showed that during the past year over fifteen thousand children had received medical treatment at the institution. It was stated that the commodious new building now in course of completion would be formally opened in a short time. We regret to state that the financial condition of this excellent charity is not so satisfactory as it deserves to be. We would strongly urge its claims for support. The charitable public could not easily find a worthier object on which to bestow their donations than the Children's Hospital.

THE Warminster magistrates a few days ago fined a gentleman £3 and costs for sending away from his house in a cab a servant who was suffering from scarlet fever. It was alleged that the offence was committed by the defendant's governess in his absence from home. We commend the decision of the magistrates, and hope this case will act as a warning throughout the country. It cannot be permitted that the health of the people should be imperilled by the recklessness of a few persons, no matter what their position in life.

DR. CORFIELD requests us to state that his course of lectures on Hygiene and Public Health, at the University College, London, will be commenced on Tuesday, May 25th, on which occasion he will deliver a public introductory lecture.

AT a meeting held last week of the representatives of the various local governing bodies of Cleveland, Dr. Thorne Thorne, one of the medical inspectors of the Local Government Board, reported that infectious diseases had been very prevalent in the town for some time past, and that the death-rate had been extremely high for a rural district. This he attributed entirely to preventable causes, one of the worst of which was the midden privy system. Many of the wells were contaminated with sewage. Dr. Thorne recommended the pail system for privies, freer ventilation of the sewer, and strongly urged the erection of cottage hospitals for isolating cases of infectious disease.

SCARLATINA and whooping-cough are still fatally prevalent in Scotland, these diseases contributing a considerable percentage of the whole mortality. The deaths from inflammatory affections of the respiratory organs, consumption and croup, have also been numerous. In March last the mortality ranged from the annual rate of 25 deaths per 1000 in Aberdeen to 35 in Glasgow. The number of violent deaths attributed directly to intemperance seems to have been rather high within the month.

DR. MAYO and his colleague, the recently appointed colonial surgeons at Fiji, are likely, as soon as they arrive, to have plenty of work to do. Recent advices from the island state that Cacobau and his son were attacked by measles on their return from Sydney, and that a large number of fatal cases had occurred among the natives. It would appear that, after all, the "palmy prospect for Fiji," indicated in THE LANCET of April 3rd, is not likely to be realised.

THE annual meeting and dinner of the Birmingham Medical Benevolent Society will be held at the Hen-and-Chickens Hotel on Friday, May 28th, at three o'clock, under the presidency of Mr. J. V. Solomon.

DR. FRANKLAND reports that the London water-supply for April showed a marked improvement, and that for the first time since May, 1874, all the samples exhibited the results of efficient filtration.

Analytical Records.

OREZZA WATERS.

WE have received a few sample bottles of Orezza, which is described as a ferruginous acidulated mineral water, imported direct from springs in Corsica. So little is known by English people generally of this little island, that it must be news to many to hear that its springs are numerous and of infinite variety. An analysis of Orezza waters showed them to be chiefly remarkable for containing large proportions of carbonic acid and of the carbonates of iron and manganese. These constituents would indicate the use of the water in many conditions of the system. It is pleasant and refreshing to the taste, and will in all probability become popular. It is imported by the Vichy Waters Company, of Margaret-street, W.

KOUMISS.

MESSRS. EDWIN CHAPMAN & Co. have submitted several varieties of Koumiss for our inspection. There can be no doubt of the increasing popularity of fermented cow's and mare's milk in this country, both as a medicine and as a pleasant and nourishing dietetic article. The immunity from consumption undoubtedly enjoyed in certain parts of Central Asia has long been ascribed to the beneficent agency of koumiss, which is a favourite food of the inhabitants. That it does prove beneficial in tubercular and wasting diseases

and in dyspepsia has been so frequently asserted in European professional journals that it is at least worthy of a fair trial. The bulk of the samples we have received had been previously analysed by Dr. Hassall, whose report is appended. The purity and composition of the several varieties are vouched for.

SUGAR-COATED PILLS.

WE have received several samples of sugar-coated pills from Mr. Hampson, Chemist, St. John-street-road, Islington. They seem to be prepared with great care and nicety. The taste and smell of the ingredients are completely masked. Even a pill containing assafœtida is rendered scentless by the process. We can testify that they are therapeutically active.

THE OUTBREAK OF ENTERIC FEVER IN MARYLEBONE AND THE ADJOINING PARTS OF LONDON IN 1873.

THE recently published Report of the Medical Officer of the Privy Council contains the official account of the investigations of Mr. J. Netten Radcliffe and Mr. W. H. Power into the extent and origin of the Marylebone epidemic. Many of the facts which we published in this journal at the time of the outbreak have been confirmed, and others have been added, which serve fully to clear up what little doubt remained upon the subject. The Report shows that the outbreak was strictly localised to Marylebone and its neighbourhood, and that at the time of this localised outbreak the metropolis as a whole was singularly free from fever. The outbreak occurred in the months of July and August, 1873, and was at its maximum in the last fortnight of July and the first week in August: 244 cases occurred, and, with the exception of 26, they were all in the households of well-to-do and wealthy people. Of the 244 cases, 218 were in households which consumed milk from a particular service of a particular dairy. Of 88 infected streets this particular milk-supply went into 77, and of 132 infected houses in the 77 streets in which fever occurred, 14 only did not receive this milk-supply. In 118 houses out of 760 taking this milk there were 218 cases of enteric fever, while in the 14 houses out of (in round numbers) 23,000 in the same area, not taking the milk, there were 14 cases only. Notwithstanding the high degree of probability which these figures give to the milk theory of infection, there were certain facts which seemed to militate against it. These were:—

1. The exemption of certain families, 63 in number, not living within the area of the outbreak, but within the district of a branch dairy in Pimlico, who received their milk from the implicated source.

2. The freedom from the outbreak of the milk-carriers engaged in the service of the dairy.

3. The entire escape of many streets within the area of the outbreak to which the service of the particular supply extended.

4. The irregular distribution of cases within the area of the milk-supply, particularly the marked predominance of cases in some streets, as compared with others.

From these facts it was evident that not all the milk supplied by the implicated dairy (which derived its milk from several farms) was infected. Happily it was possible to distinguish between the varieties of milk supplied because the dairy was in the habit of selling so called "nursery milk,"—milk, that is, derived from particular animals, and which was sent in sealed cans to the houses of the purchasers. This "nursery milk" formed $\frac{1}{6}$ of the whole supply of the dairy, and it was furnished by a particular farm, Chilton Grove, near Thame, in Oxfordshire, which in addition supplied one-sixth of the ordinary milk issued by the dairy.

Early in the epidemic it had been noticed that the denizens of the nursery, women and children, chiefly suffered, but later this limitation of the attack was not so noticeable. Of the 244 recorded cases, 119 were women and 96 children, only 29 were adult males. It was at first thought that the nursery milk was alone infected, but further investigation showed that the ordinary milk was not free from suspicion; but the ultimate result obtained was that the ordinary milk was infected only to such extent as nursery milk might have become mixed with it.

The investigation of the history of this nursery milk was the means of answering some of the objections mentioned above. In the first place, it was proved that none of the 63 families living in Pimlico, who were exempt from fever, were supplied with nursery milk; but, strangely enough, almost the whole of the ordinary milk from Chilton Grove was distributed among them. "*This fact excludes from suspicion of infection the ordinary milk coming from the farm in question.*" This "nursery milk" theory explains also the exemption of the carriers, for no nursery milk was ever or hardly ever given to them, and, the cans being sealed, they could not help themselves.

When, towards the end of the London season, many of the customers of the dairy left town, there was a surplus of "nursery milk," owing to the diminished demand, and this surplus was dealt with in the way following: It was either (*a*) "set" (as the phrase is) for cream, or (*b*) it was sent into the shop to be sold across the counter; or (*c*) it was used to supplement the general supply. If used in the latter way it was not added to the general bulk of the milk, but was merely added to one of the last-served carrier's cans. These facts serve, in the opinion of the authors of the Report, to meet the third and fourth objections to the milk theory. Thus we are furnished with a proof, amounting almost to positive certainty, that the outbreak of fever in question was attributable to an infected milk-supply. The next point taken up was the mode of infection of the milk. The whole of the suspicion fell upon Chilton Grove farm, and the following are the facts which chiefly serve to elucidate the question:—

The occupier of the farm died suddenly on June 8th, in the fourth week of an attack of enteric fever. During the last week of illness the evacuations of this patient had, in accordance with the advice of his medical man, been buried in an ash-heap beyond the precincts of the farm-buildings.

On August 12th the son of this patient was seized with enteric fever, and passed through a mild but well-marked attack. Neither wife nor daughter, nor any of the labourers or their families, suffered from fever.

The water for dairy purposes was drawn from a well situated in a yard adjoining the farm-house. The occupants of the house had ceased to use this water for drinking purposes, because of its unpleasant nature, and a chemical analysis afforded evidence of "recent contamination from sewage or surface drainage." Digging around this well disclosed the fact that no leakage into or from the privy or house-drains had taken place, but it was discovered that the liquid manure from the piggeries, twenty-five feet distant, had soaked gradually through the intervening clay soil (which was reduced thereby to a pulp), and leaked freely into the dairy-well. In close contiguity to the piggery-wall, along which the soakage took place, was the ash-heap in which the deceased man's evacuations had been buried. "*Thus,*" say the reporters, "*by an unhappy and altogether unforeseen chance, and in carrying out precautions to obviate any possibility of mischief, the matters from which mischief was most apt to arise were deposited in perhaps the only spot on the farm premises where they would certainly find their way into the water used for dairy purposes.*"

As to the actual mode in which the milk became infected by the water, or how it was that the "nursery milk" suffered while the rest apparently escaped, these the authors consider are matters concerning which it is, in the very nature of things, impossible to know the truth. They content themselves with drawing the following conclusions:—

"1. The outbreak of enteric fever which forms the subject of the inquiry was caused by milk infected with enteric fever material.

"2. That this milk came from a particular farm.

"3. That the water used for dairy purposes on the farm contained excremental matters from a patient suffering from enteric fever immediately before and at the time of the outbreak."

The experience gained by the investigation of this case has forced upon the attention of Mr. Radcliffe and Mr. Power the fact, that in too many of our dairy farms the source of the water-supply is very far from being what it should be; that sewage contamination is possibly the rule rather than the exception, and that all the elements for an accident such as

that which is the subject of the paper are but too frequently present.

"Until this habitude of using, and indifference to the use of, foul water or of water liable to be fouled for dairy purposes, is put a check to, probably the most important source of infection of milk with enteric fever material will hardly be removed. Such infection is, indeed, to be held as one of several highly probable evils arising from this source. One of the results of this inquiry which has been most forcibly impressed upon the reporters, is that the use of water for dairy purposes fouled after fashions such as have been described, may, by inducing changes in the milk, particularly under high ranges of temperature, play an influential part in determining fatal infantile diarrhœa."

This Report is one well worthy of careful perusal by every health officer, and is a model in its way of what such reports should be. We have been enabled only to give a faint outline, and such as can convey no adequate idea of the hard work, the patience, and the skill which have been employed upon it. We congratulate Mr. Radcliffe and Mr. Power on the successful issue of their labours.

THE "BESSEMER."

THE daily journals have already acquainted the public with the fact that the saloon steamer, designed conjointly by Mr. Bessemer and Mr. Reed, crossed from Dover to Calais on Saturday, on an official trip, with a large party of scientific and other distinguished men on board. All professions connected directly or indirectly with marine matters were represented on the occasion, so that the peculiar properties of hull outline, machinery, ventilation, and, chiefest, of the swinging saloon, might be fully and fairly tested by competent critics. As to the first two items above-named, it is not our province to say anything. Great care appears to have been taken to secure the comfort of passengers by special ventilating arrangements for the saloon as well as the other parts of the ship. They are somewhat similar to those described last week as existing on board H.M.S. *Assistance*, and an unusually large number of small syphoned up-casts are provided to carry off foul air from the nethermost parts of the ship. The swinging saloon is specially ventilated by ground-glass louvred windows, that extend along each side close to the roof. Although at the time of the passage across, the thermometer in this gorgeous apartment stood only at 65° Fahr., we believe that, for the exigencies of the summer months, further provision for out-casts will have to be made. It is necessary, too, to draw attention to the fact that the stokeholes were excessively and most unnecessarily hot. The thermometer at the forward door of the aft stokehole (which should serve as down-cast) stood at 96° Fahr., and it is evident that both forward and aft the "plénum" supplies are not sufficient. But all these defects are remediable, and can soon be remedied by the well-known ingenuity of the Constructor and his staff. Of Mr. Bessemer's "spécialité," the swinging saloon, nothing can be said, because the apparatus was not in working order, and indeed under any circumstances the sea was so smooth that no crucial trial could have been made. Looking for a fair and complete trial at no distant date, we do not care to canvass the principle of the invention, or the probabilities of success, but may record that in point of luxurious accommodation and all sorts of comfortable appliances, the *Bessemer* rivals any passenger ship afloat. The courteous hospitality of the three companies resulted in a very delightful trip, and will afford to all the guests many pleasant reminiscences.

DR. LYON PLAYFAIR'S VIVISECTION BILL.

THE Bill introduced by Dr. Lyon Playfair, Mr. Spencer Walpole, and Mr. Evelyn Ashley, "to prevent abuse and cruelty in experiments on animals made for the purpose of scientific discovery," has been printed. It proposes to enact that painful experiments on living animals for scientific purposes shall be permissible on the following conditions:—"That the animal shall first have been made insensible by the administration of anæsthetics or otherwise, and shall continue to be insensible during the whole of such experiment; and that, if the nature of the experiment be such as to seriously injure the animal, so as to cause to it after-suffering, the animal shall be killed immediately on the termination of the experiment." Experiments without the use of anæsthetics are also to be permissible, provided the following conditions are complied with:—"That the experiment is made for the purpose of new scientific discovery, and for no other purpose; and that insensibility cannot be produced without necessarily frustrating the object of the experiment; and that the animal should not be subjected to any pain which is not necessary for the purpose of the experiment; and that the experiment be brought to an end as soon as practicable; and that if the nature of the experiment be such as to seriously injure the animal, so as to cause to it after-suffering, the animal shall be killed immediately on the termination of the experiment. That a register of all experiments made without the use of anæsthetics shall be duly kept, and be returned in such form and at such times as one of her Majesty's principal Secretaries of State may direct."

The Secretary of State is to be empowered to grant licences to persons provided with certificates signed by at least one of the following persons:—The President of the Royal Society, the Presidents of the Royal Colleges of Surgeons or of the Colleges of Physicians in London, Edinburgh, or Dublin, and also by a recognised professor of physiology, medicine, or anatomy.

Correspondence.

"Audi alteram partem."

EXAMINATIONS IN PHYSIOLOGY IN THE UNIVERSITY OF GLASGOW.

To the Editor of THE LANCET.

SIR,—It is to be regretted that in his comments on the reports of the visitations of the University of Glasgow, in THE LANCET of to-day, Professor Buchanan has not quoted and indicated the passages of the reports to which he objects, and to which he replies by "an unqualified contradiction," as this might have enabled your readers to judge how far he has really given a contradiction to any part of them.

With regard to the statement of facts made in the report to which he more particularly alludes—viz., that there was no examination in practical histology, and that there were no means, microscopical or other, of ascertaining the practical acquaintance of the candidates with the subject of physiology, Professor Buchanan does not in the least contradict this, but inferentially confirms it, and explains it by showing the difficulties he has encountered in the practical teaching of physiology from want of the necessary apparatus. It might have been thought, therefore, that our mentioning those deficiencies, and so calling the attention of his University to them, would have been acceptable, rather than objectionable, to him, and would be a means of assisting him to obtain those appliances the want of which he so much laments.

With regard to the opinion expressed, that the questions in physiology were not calculated sufficiently to test the knowledge of the candidates, and that the estimate of the answers was much above their merit. This being an opinion does not admit of contradiction, and can be shown to be incorrect only by reference to the questions, the answers, and the marks assigned to the answers.

With regard to the five questions appended to Professor Buchanan's letter as having been put by him to the candidates for M.B. of the present session, and the request for my answers to them, I may remark that my business as a visitor is not to answer questions, but to criticise them; and certainly these questions are open to the *primâ facie* objection that three out of the five are upon the circulation of the blood—a subject upon which the Professor has recently

given to the world a treatise evincing no little research and ability, and therefore one upon which it might be expected that his own pupils would be likely to be prepared. It appears that answers (more or less complete answers, I presume) to two of these questions and one of the others satisfied the requirements. These questions are, therefore, not without their value as affording some confirmatory evidence of the correctness of the view expressed in another part of the report respecting the disadvantages attendant upon the system under which the examinations of the candidates in the several subjects are conducted almost entirely by their teachers in those subjects, so that the same person is practically responsible both for the teaching and the examining in a given subject; and we learn from Professor Buchanan's letter that no change has yet taken place in this method, at any rate, in the written part of the examination in physiology.

It is only right that I should add that a similar plan was followed in the other Scotch universities; but we were glad to find recently, on visiting the examinations in the University of Edinburgh, that a change has been made there, and that this year an examiner specially qualified in the subject, and in some instances not a member of the University, has been appointed to assist each of the professors, and takes an equal share with him in conducting the examination. In Aberdeen, where we found the practice differing from that at Glasgow, we learned that an addition to the staff of assistant examiners is contemplated, and I have reason to believe that there is a strong desire in Glasgow to do the same, and to adopt a plan similar to that inaugurated this year in Edinburgh. It would be very satisfactory to learn, before another examination takes place, that something of the kind had been done, and that action had thus been taken in accordance with the spirit of a resolution upon this subject passed at the last meeting of the General Medical Council.

I am, Sir, yours obediently,

Cambridge, May 8th, 1875. G. M. HUMPHRY.

To the Editor of THE LANCET.

SIR,—As a graduate of Glasgow University, and one who for the past ten years has watched with interest the progress of physiological teaching in both the London and provincial schools, perhaps you will permit me briefly to refer to the letter of Dr. Andrew Buchanan in THE LANCET of the 8th inst. regarding the report of the General Medical Council on the state of physiological knowledge displayed by the students of Glasgow University.

It is always a painful thing to oppose an old and honourable teacher; but when the interests of a University are at stake, feeling must give way. No one could hold Professor Andrew Buchanan in greater respect than I do, or be more alive to his former great ability as an operative surgeon, as well as an experimental physiologist; but it is to be feared he has allowed his laurels to fade through wearing them too long in the sun. *Troja fuit.* Physiology is now held to be of so immense importance in medical training that no school can neglect it with safety to its good name. This, I fear, Glasgow threatens to be slow of recognising; and it is solely desire for the welfare of my *alma mater* which prompts me to trouble you with this letter. There can be no doubt that for many years physiology has not had fair play in Glasgow. I sat through the usual course regularly and attentively, but I learned little physiology—an experience which must have been common in the class. Kirkes was the source generally of our examination papers. This is not the first time I have so expressed myself, for both before and after the report in question I have discussed the matter with one of the University teachers, using language much more severe than that advanced by Dr. Humphry and his *confrères*. Dr. Buchanan's own words are—"I ask Professor Humphry if it be possible to teach practical physiology without instruments, without a cabinet in which to place the few that could be procured, without a laboratory, without funds, and without an assistant?" Here we have the whole question in a nutshell. "All this," he goes on to say, "I did for five-and-twenty years." I simply ask, How could he?

I am, Sir, yours respectfully,

Caledonian-road, London, May 10th, 1875. J. CHALMERS, M.D.

CROUP AND DIPHTHERIA.

To the Editor of THE LANCET.

SIR,—The following brief remarks were addressed by me to the West Kent Medical Society on Friday evening last, and may, although feeble and imperfect, help forward the true interpretation of diseases on which apparently there is great want of agreement. They are the result of personal observations, and were supported by narrated cases.

1. Croup, properly so called, is an acute inflammatory disease of the windpipe, chiefly attacking young children, and distinguished by difficult and noisy breathing, with the formation in the larynx, trachea, and bronchi of a false plastic membrane. It is never endemic, epidemic, or infectious; is very fatal, and now rarely seen. The exciting cause appears to be cold. In fatal cases where life has been prolonged for a few days, the false membrane shows signs of organisation; and in other cases, during the progress of recovery, membrane has been expelled containing, as far as could be made out, traces of new bloodvessels. It would therefore appear that this plastic membrane undergoes a change somewhat like the plastic effusion in the pleural and abdominal cavities.

2. Croup may be simply spasmodic and due to dental or stomach or intestinal irritation—a reflex phenomenon, the removal of which is followed by a total cessation of noisy breathing. In some families there appears to be a common tendency to this variety of so-called "croup."

3. Another form of croup is that known as laryngismus stridulus, dependent apparently on strumous enlargement of the cervical and thoracic glands, which press on or irritate the recurrent laryngeal nerves.

Now, diphtheria, although occasionally accompanied by noisy breathing and improperly, called "croup," it is submitted, is truthfully a special general disease, endemic, not epidemic, neither infectious; marked by the formation of membranous spots or membrane on the soft palate, fauces, and, in the worst cases, in the larynx, trachea, and bronchi, often extending to the minutest ramifications of the lung-tissue. Great prostration of the vital powers marks its accession, with frequently a discharge of ichor from the nostrils and swelling of the cervical glands, indicating too plainly that there is blood-poisoning. In the earliest stage of the disease there is usually found albumen in the urine. The so-called membrane never shows signs of organisation as in true croup. In cases of a less grave character, even with considerable membrane formation in the throat, the absence of albumen offers the most favourable indication, and justifies a hopeful termination, particularly if the patient be removed from the infected house.

When, however, this disease appears in a household consisting of several members, and especially of young children, the rule is for the youngest to die first and the law of attack and severity to be measured by age—the elder members resisting the poison the longest. Unless, however, a speedy removal is effected to more healthy surroundings, death will carry off many in the family, and those who escape the final grasp will often suffer from a long convalescence, or, it may be, of paralysis or some equally grave invalidism.

These very brief notes, ten minutes only being allowed to recite them, bring before us, it appears to me, diseases as different as erythema, measles, scarlatina and erysipelas, all of which have one common symptom, that of skin-redness. No one regards these as identities. Neither is it philosophic to call all diseases which are characterised by noisy breathing, croup.

True inflammatory croup, the only true form of croup, is the result of sthenic conditions, and requires active treatment. The patient ought not to be removed from the house, but kept in bed. Neither do other cases, as a rule, follow its accession.

If we desire that the names of diseases should convey a knowledge of the nature of the malady, the two forms of so-called spasmodic croup should receive, and be generally known, the one as *spasm of the windpipe* and the other as stridulous breathing—thus getting rid of croup as applied to them.

In diphtheria the most opposite treatment is called for; and the step most urgent of all is the immediate removal of all the inmates from the infected house. All other treatment should be regarded as of no avail until the patients have been taken away from the poisoned dwelling. In every case of

diphtheria where careful inquiry has been made insanitary conditions have been found to exist, and in no case could the spread of the disease be traced to personal contact.

I remain, Sir, &c.,

Blackheath, May 10th, 1875. WILLIAM CARR, M.D.

TEMPORARY DELIGATION OF ARTERIES.

To the Editor of THE LANCET.

SIR,—In justice to the memory of Scarpa, I ask you to correct a misstatement of his method of the temporary deligation of large arteries with which I am credited in your report of the discussion upon Mr. McGill's case, read at the Royal Medical and Chirurgical Society on the 27th ultimo.

What I really said was to the effect that Scarpa, in order to prevent any injury to the inner and middle coats of the artery, used a broad ligature composed of several waxed flattened threads, and he tied this upon a small roll of linen, placed between the artery and the knot, only just long enough to reach beyond the grasp of the ligature. As Scarpa thought it of great moment to avoid severance of the artery by the ligature, he untied and removed this on the second or third day, before which time he thought that ulceration of the artery would not have happened.

I am, Sir, yours obediently,

Old Burlington-street, May 11th, 1875. J. W. HULKE.

SKETCHES OF CONTINENTAL HOSPITALS.

(By our Special Correspondent.)

VIII.—BERLIN.

THE capital of North Germany increases in population and in size at such a rate that it is difficult for the necessary supply of hospital accommodation to be provided by the authorities without going to the outskirts of the city for fresh sites for hospitals. The Charité, large as it is, needs extension; the University Hospital is so old and ill-constructed that it will be pulled down as soon as a suitable piece of ground can be found in the centre of the city so as to be readily accessible to the students. The newer hospitals, as the Bethanien and the Augusta Hospitals, are at opposite parts of the city, and a long way from the centre; while the newest, or Stadt Hospital, which is just about to be opened, is quite in the suburbs. The Charité and the University are the only two of the hospitals at which clinical instruction is given, and at which students are expected to attend the practice; hence there is a great crush at both of them, while the other hospitals, containing also ample material for the education of students, and ably officered by efficient physicians and surgeons, are of no value as seats of clinical teaching. There is a special hospital for Jews, endowed and supported entirely by members of that community; here also no students are admitted. Is it from this paucity of practical clinical material that Berlin has a much smaller number of students than even Leipzig or Wurzburg, while she has less than one-third of the number of the University of Vienna? Only 333 medical students entered in the winter of 1873 at Berlin, while Leipzig had 599, Wurzburg 499, and the capital of Southern Germany had about 1000. No one can account for it by the greater intellectual or scientific superiority of the professors of those universities, for none of them could adduce more brilliant names among their professorial staff than those of Du Bois-Reymond, Frerichs, Langenbeck, Virchow, and Helmholtz. Either the University Hospital should be rebuilt on a large scale near the University itself, or the medical department of the University should be shifted altogether to the neighbourhood of the Charité Hospital, and the Augusta Hospital, which is not far distant, and which might readily be extended, be made available for clinical purposes. That the latter proposition may be carried out seems not improbable, for a large building is being erected on the banks of the river Spree, not far from the Charité, which will comprise the physiological laboratories, &c., of Du Bois-Reymond, and also those devoted to physics, under the control of Helmholtz and Dove.

The medical klinik at the University Hospital is under Prof. Traube; the surgical under Prof. von Langenbeck. A numerous class of students is daily seen here following the practice of these two eminent men. The wards are small and crowded, and therefore clinical instruction is chiefly given in the lecture theatres. The surgical theatre, which is noteworthy as being the scene of Dieffenbach's labours, and the spot where he died, is lofty and well lighted. Langenbeck lectures and operates daily from 2 to 3 P.M., but the number of special operation cases sent from all parts to this celebrated surgeon is so great that he often goes on seeing patients, teaching students, and operating for three or four hours. Resections of all joints are seen in profusion, and, as far as possible, are conducted subperiosteally. For the wrist, Langenbeck makes a single straight incision at the back of the hand, beginning at the base of the first metacarpal bone, and going downwards to the wrist-joint, he so misses the princeps pollicis artery; he then takes away the first row of metacarpal bones, afterwards the second row, and, if necessary, the heads of the metacarpal bones; through the free opening which now exists he is able to push out readily the radius and ulna, and to cut their ends off with an ordinary saw. The results, as seen in numerous cases both at this hospital and at the Jacob's Hospital, were admirable. The wounds after resections are washed out with a weak solution of chloride of lime, but Lister's plan of treatment is never adopted. Facial plastic operations are great favourites of the Professor, and in his wards may be seen cases in which almost every feature of the face has been reproduced by operation. Prof. von Langenbeck, during his twenty-five years of service at Berlin, has educated a host of surgeons of celebrity, perhaps the most celebrated being Billroth, the present Professor of Surgery at Vienna.

Traube and Langenbeck are also respectively physician and surgeon to the Jacob's Hospital, a new building with 300 beds, and with every modern convenience for the comfort and well-being of the patients. Here may be seen some of Langenbeck's more successful resections, performed as they are under better hygienic conditions than at the University Hospital.

The Augusta Hospital resembles our own hospitals in being supported mainly by voluntary contributions, one of its most liberal supporters being the Empress, after whom it is named. It is a new hospital, containing 90 beds, half medical and half surgical, and consists of a central brick building, with six pavilions built of wood on a brick basement, and arranged on the barrack principle. At one end of each ward, containing 14 beds, is a nurses' room and small kitchen, and at the other a corridor, which is glazed, and can be completely closed, in which the patients may sit or walk. In the gardens around, which are extensive, there is also one wooden building for males, and another for females, so that cases of contagious diseases may be isolated from the rest of the patients. New pavilions are about to be erected, and probably this hospital will soon be greatly increased in size. Dr. Fränkel is the physician, and Dr. Küster the surgeon to it. The latter has been remarkably successful with the operation of transfusion—of twenty-four cases not one has been fatal. In six of the cases eight ounces of blood were taken from the human subject, and in the remaining eighteen six ounces of blood, taken from lambs, were injected; in all the cases Schliep's apparatus being used. Many interesting cases resulting from the war are seen here. One man was shown who was shot through the left side of the chest in 1866, the ball passing through the lung, and making its exit near the spine. The ends of two ribs had been resected, and drainage-tubes employed to relieve the empyema and to cure the pulmonary fistula, and the patient was almost recovered. Another case of rarity was that of a woman for whom lumbar colotomy had been performed to relieve a syphilitic stricture of the rectum.

ROYAL COLLEGE OF SURGEONS IN IRELAND.—At a meeting of the Council, held on Tuesday, the 4th inst., the following were elected examiners for the ensuing year:—Examiners for Letters Testimonial and Fellowship: John Barker, Robert McDonnell, Benjamin W. Richardson, Edward A. Stoker, Edward S. O'Grady, William H. O'Leary, Henry Gray Croly, William Thomson. Examiners in Midwifery: William Roe, John Rutherford Kirkpatrick, Henry Croly. Examiners in General Education: Henry J. Tweedy, Stewart Woodhouse, Frank J. Davys.

Obituary.

DR. VANS BEST.

WE regret to have to record the death of Dr. Vans Best, which occurred recently at Hyères, in the South of France, whither the deceased gentleman had gone for the benefit of his health.

Dr. Vans Best was in his thirty-ninth year, and his future life bid fair to be honourable to himself and useful to his fellows, had it not been cut short.

He was the son of Mr. Thomas Best, banker, of Aberdeen, and in the year 1857 he graduated in medicine at the University of that city, and gaining shortly afterwards a high position on the list of successful candidates for Indian medical appointments, he sailed for India, where he practised for eleven years as an army surgeon. He became first surgeon to the European General Hospital, and subsequently staff-surgeon in the Bengal Army. Towards the close of the mutiny he served with the Naval Brigade, and during the war with China he accompanied the field forces and was placed in charge of hospitals. For these services he was awarded a medal, and after his return to India he became sole officer of the European Depôt Hospital and the Female Hospital of Raneegunge, Bengal. He was subsequently employed on the Trans-Indus frontier with the cavalry forces. During his Indian career Dr. Vans Best made a name for himself, not only in professional matters, but in matters of arrangement, organization, and discipline, and was officially thanked for his valuable services during an epidemic of cholera. This career of increasing distinction was unfortunately cut short by failing health, and at its close he was appointed by the Viceroy to accompany Lady Lawrence and suite to England, where he arrived early in 1867.

In 1868 he settled in Aberdeen and resumed medical practice, and soon became known not only as a practitioner, but as a man of no small scientific attainments. In 1873-74 he was entrusted with the class of Midwifery in the University during the unavoidable absence, through illness, of the Professor, Dr. Inglis.

Dr. Vans Best was the author of several papers on medical subjects, and in 1871 contributed to our columns an able paper on the Radical Cure of Hernia; and again, in 1873, he contributed a case of Atresia Vaginæ.

Since his return from India, Dr. Best had suffered from renal disease, and at the beginning of last winter started for the South in the hope of arresting or alleviating his malady. This, however, was not to be, and he has left a widow and two children, as well as a numerous circle of friends, both within the profession and without, to mourn his untimely loss.

BENJAMIN WALKER, M.R.C.S.

MR. WALKER was the son of an officer of Chelsea Hospital, and born there on the 6th April, 1814. He was articled to Mr. Hartshorn, one of the surgeons to that institution, became a student of St. George's Hospital, and obtained the diploma of the Royal College of Surgeons, England, May 3rd, 1875. After being a short time in private practice he was, in the year 1853, appointed one of the medical officers of Chelsea Hospital, and remained so until August last, when he retired, to the great regret of his colleagues. He was a member of the Chelsea Literary Institution, and usually attended the weekly lectures there. He did so on the evening of Friday, the 16th April, but, feeling unwell, returned in a cab to his residence, No. 5, Queen's-road West, Chelsea, where he died from rupture of one of the large vessels of the heart in about three minutes, at the age of sixty-one. He was a man of most amiable disposition, cheerful in manners, and had endeared himself to a large circle of friends, by whom his sudden loss is most acutely felt.

ROBERT RANKINE, M.R.C.S.

MR. RANKINE was born at Bintaskine, near Falkirk, Stirlingshire, educated in Edinburgh, and became M.R.C.S. Eng. in 1814. He spent twenty-two years of his life in India, and during that time wrote "The Medical Topography of Sarun," published by order of the Government. He was on active service during the first Burmese war, wherein he was slightly wounded, and at the close of which he received a medal from the Queen. In 1842 he returned to England, and a few years afterwards became J.P. for the county of Middlesex, continuing such up to the time of his decease, which took place on April 21st, at 44, Porchester-terrace, Hyde-park, at the age of eighty. His death is as universally regretted as his life was respected.

PARLIAMENTARY INTELLIGENCE.

Week ending May 13th.

HOUSE OF COMMONS.

NAVY SURGEONS.

Replying to Mr. O'Leary, Mr. HUNT said the number of naval surgeons who had resigned, during the last five years, was twenty-five. The cause of resignation had not, in any case, been assigned to dissatisfaction at the treatment which the medical officers of the navy had experienced. There were six tenders of resignation that were refused in the first instance. It was not true that the majority of surgeons appointed within the past five years to the naval medical service had been mainly those rejected by the examining boards of the army and Indian medical service. No person so rejected had been received into the naval service.

THE MEDICAL EXAMINING BODIES.

Mr. WADDY asked the Vice-President of the Committee of Council on Education whether the Government had any information from the Medical Council of the success of attempts on the part of the medical examining bodies in the three divisions of the kingdom to form Conjoint Boards for giving diplomas that should constitute a complete qualification in all branches of the medical art; and whether the Government, in the event of its not receiving or of not having yet received satisfactory information, was likely to bring in a Bill, or otherwise initiate legislation, with a view to remedy the present system of half-qualifications by nineteen competing bodies.

Lord SANDON said—We have no official information from the Medical Council on the subject of the hon. gentleman's question, but I have reason to believe that attempts are still being made to form Conjoint Boards for giving diplomas that shall constitute a complete qualification in all branches of the medical art. How far these attempts may be successful we cannot yet say, but I may point to the Society of Apothecaries' Act of last session, and to the Bill respecting the College of Surgeons now before the House, as proofs that the matter is not being left alone by the medical bodies. The attention of Government has only quite recently been called to this subject, which is a very large and complicated one. I am not prepared, therefore, at present to state the views of Her Majesty's Government respecting further legislation.

SALE OF FOOD AND DRUGS BILL.

A considerable amount of progress was made with this Bill in committee on the 6th and 11th inst. When Clause 9 came on for discussion a great deal was said about the inadequate number and incompetency of analysts throughout the country, and an amendment was proposed by Mr. Sandford to the effect that county magistrates shall, when the Bill becomes law, be compelled to appoint analysts. Sir Henry Peek again urged the advisability of employing the Somerset House officials. Dr. Playfair, commenting upon the fact that the Bill contained no proper security that the analysts should be competent, declined to support the "county" amendment, because London and the boroughs were well able to find proper men for the work. The amendment was eventually withdrawn, and a very important insertion was moved by Dr. Playfair, and assented to by Mr. Sclater-Booth, to the effect that no persons shall be appointed who are engaged in the trade of buying or selling drugs or any article of food.

ARMY MEDICAL SERVICE.

Dr. LUSH gave notice that on an early day after the Whitsun recess he would call attention to the medical officers of the army with respect to their position and emoluments, and move a resolution thereon.

VIVISECTION.

Dr. PLAYFAIR obtained leave and brought in a Bill to

prevent cruelty and abuse in the experiments made on living animals for the purpose of promoting scientific discovery.

INFANTICIDE BILL.

This Bill was read a second time.

Medical News.

ROYAL COLLEGE OF SURGEONS OF ENGLAND. — The following gentlemen passed the primary examination in Anatomy and Physiology at meetings of the Court of Examiners on Tuesday and Wednesday last:—

J. F. Witz and W. Devenish, Dublin; W. H. Maberly, J. J. Routh, H. O. Westwood, and J. Greasley, Edinburgh; C. E. Richmond, C. F. Porter, A. T. Winterbottom, J. E. Edington, W. H. Clarke, E. A. Aldridge, and C. F. Diggle, Manchester; A. W. Stone, A. S. Jenkinson, W. H. Elam, W. C. Clarkson, and A. Fawcett, Leeds; A. B. Brown, Sheffield; H. D. Crook, Bristol; H. I. Capon, S. D. Darbyshire, J. H. Clegg, W. H. Gaze, W. H. Goodchild, C. Bullock, F. F. L. Robertson, A. J. Bathe, E. F. Prior, and P. Tarleton, St. Bartholomew's Hospital; B. F. Giles, P. O. Haynes, C. J. Plummer, H. Peskett, F. Waddington, G. W. Butler, and A. Smart, Guy's Hospital; D. J. Jones, Edinburgh and Liverpool; E. Smith, F. W. Wiles, W. P. Jago, and D. H. Davy, London Hospital; O. M. White, St. Mary's Hospital; C. T. Jones, C. J. Watson, and C. F. Budler, University College; J. H. Hough, St. Mary's Hospital and Cambridge; G. A. Cardew, Birmingham; W. J. V. Harle, Cambridge and University College; J. Wood, Birmingham and University College; A. T. O. White, St. George's Hospital; H. R. O. Cross, King's College; G. R. E. Bonsall, Middlesex Hospital.

APOTHECARIES' HALL. — The following gentlemen passed their examination in the Science and Practice of Medicine and received certificates to practise on May 6th:—

Brenchley, Algernon Dutton, Denmark-hill.
Jackson, Philip John, Merton, Surrey.
Neylan, Joseph, Ennis, Ireland.
Snell, Edmund George Carruthers, Stepney-green.

The following gentlemen passed the Primary Professional Examination on the same day:—

Edward Berdoe, London Hospital; Henry Foster Burnes, Queen's University in Ireland and the London Hospital.

MR. PEARSON R. CRESSWELL, F.R.C.S. Edin., has been appointed High Constable of Dowlais.

PRESENTATION.—The students of the Dental Hospital presented to David Hepburn, Esq., on Tuesday, May 4th, a handsome silver salver as a mark of their esteem on the occasion of his resigning the post of house-surgeon.

SOCIETY OF MEDICAL OFFICERS OF HEALTH.—The next meeting, being the last of the session, will be held on Saturday, May 15th, at 7.30 P.M. Dr. Francis T. Bond, medical officer of health for Gloucestershire Combined Sanitary District, will read a paper entitled "The Public Health Bill in its Relations to the Sanitary Requirements of Rural Districts."

EDINBURGH UNIVERSITY CLUB. — The Quarterly Dinner of this club was held at St. James's Hall on Monday evening, the 10th inst. Dr. Maclean, C.B., of the Royal Hospital at Netley, occupied the chair. There were numerous guests present on the occasion, and amongst them were the Rev. Daniel Moore, Mr. Winter Jones, Mr. Adams, and Mr. Carl Haag.

SOCIETY FOR THE RELIEF OF WIDOWS AND ORPHANS OF MEDICAL MEN.—The annual meeting of this Society was held on Friday, April 30th, by the kind permission of the Royal Medical Chirurgical Society, in their rooms, 53, Berners-street. The chair was taken at half-past 8 o'clock by the President, Sir George Burrows, Bart. From a summary read by the Secretary it was shown that the total receipts available for payments for the year 1874 amounted to £3065 15s., and the total grants and expenses for the same period to £3174 14s., exhibiting an excess of expenditure over receipts of £108 19s. The ordinary grants for the year were £2601 10s., an increase of £71 10s. on those of 1873. The grants had been augmented at Christmas to the amount of £329, caused by a donation of £5 to each widow, £2 to each child, and £5 to each of the recipients from the Copeland Fund, making in all £2930 10s. granted during the year. Nine members had died (four leaving their widows to the charge of the Society), four had resigned or ceased to be members, and twelve new members had been elected, leaving the list of members at 396—one less than in 1873.

Medical Appointments.

ANDERSON, Prof. A. G., has been appointed Public Analyst for the Parish of Paddington, vice Hardwicke, resigned: £150 per annum.

BASS, C. W., M.R.C.S.E., has been appointed House-Surgeon to the Beckett Hospital and Dispensary, Barnsley, vice Shaw, resigned.

BATELY, J., L.R.C.P.L., M.R.C.S.E., has been appointed Medical Officer of Health for the Great Yarmouth Urban Sanitary District.

BIRCH, P., M.R.C.S.E., has been appointed House-Physician to King's College Hospital.

BLAKE, J. F., M.B., L.R.C.S.I., has been appointed Assistant Resident House-Surgeon to the North Dispensary, Liverpool, vice Norman, resigned from ill-health.

BOWER, A. E., M.R.C.S.E., has been appointed Medical Officer for the Prees District of the Wem Union, Salop, vice Burroughs, resigned.

CHEYNE, W. R., M.R.C.S.E., has been appointed a Surgeon to the St. George and St. James Dispensary, King-street, Regent-street, vice Aspray, resigned.

CLARKE, T. E., M.R.C.S.E., L.S.A.L., has been appointed Medical Officer to the Furness Abbey Lodge Independent Order of Odd Fellows, also the Court Bowmen of Furness Ancient Order of Foresters, vice Horn, deceased.

DENNING, J. V. C., L.K.Q.C.P.I., L.R.C.S.I., has been appointed Medical Officer of Health for the Greetland Rural Sanitary District: £10 per annum.

DOVE, H., M.R.C.S.E., has been appointed Medical Officer for No. 1 District and the Workhouse of the Stow Union, Suffolk.

DUNCAN, A., M.B., M.R.C.S.E., has been appointed Resident Medical Officer to the Public Dispensary, Stanhope-street, Clare-market, vice G. H. Percival, M.B., resigned, on being appointed Medical Officer to the Biddulph-valley Coal and Iron Works, Brindley-ford.

FOWLER, J. K., M.R.C.S.E., has been appointed House-Surgeon to King's College Hospital.

GREEN, E. F. S., M.B., C.M., has been appointed a House-Physician to the Western Infirmary, Glasgow (late Assistant-Surgeon at Coltner's Ironworks), vice A. Denholm, M.B., C.M., whose appointment has expired.

GREENWOOD, C., M.R.C.S.E., has been appointed Junior House-Surgeon to the Cheltenham General Hospital and Dispensary, vice Stephens, appointed House-Surgeon and Surgeon to the Kidderminster Infirmary.

GREVILE, F. A., L.R.C.P.Ed., L.R.C.S.Ed., has been appointed a District Surgeon to the Salford and Pendleton Royal Hospital and Dispensary, Manchester, vice Bonser, deceased.

GRIFFITHS, G., L.R.C.P.Ed., M.R.C.S.E., has been appointed Medical Officer for the Western District of the newly formed Pontardaw Union; also Medical Officer of Health.

HODGES, B., M.R.C.S.E., has been appointed Medical Officer and Public Vaccinator for the Parish of Membury in the Axminster Union for one year.

HOLT, G. A., M.P.S., has been appointed Lecturer on Botany and Materia Medica at the South London School of Pharmacy until the end of the session, vice Twist, deceased.

JOB, J., M.R.C.S.E., has been appointed House-Surgeon to the London Hospital, vice Lang.

KELLY, E. H., M.D., C.M., has been appointed House-Surgeon to the Lanark Hospital, vice Greville, appointed a District Surgeon to the Salford and Pendleton Royal Hospital and Dispensary, Manchester.

KIRKWOOD, G., L.R.C.P.Ed., L.R.C.S.Ed., has been appointed Resident House-Surgeon to the Cumberland Infirmary, Carlisle, vice Skaife, resigned.

M'CALMAN, D., M.B., C.M., has been appointed a House-Surgeon to the Western Infirmary, Glasgow, vice D. Foulis, M.D., whose appointment has expired.

M'COSH, R. N., M.D., has been appointed Resident Medical Superintendent of the Royal Infirmary, Dundee, vice Sinclair, resigned.

M'KELLAR, Mr. E. D., has been appointed a House-Surgeon to the Western Infirmary, Glasgow.

M'LOUGHLIN, Mr. G., has been appointed Apothecary to the Abbeyleix Union Workhouse, vice Honnor, deceased.

MARSHALL, W. N., M.R.C.S.E., has been appointed Medical Officer of Health for the Newent Rural Sanitary District: £40 per annum.

MICHELL, G., M.R.C.S.E., L.M., has been appointed Medical Officer for the Gwennap District of the Redruth Union, vice Harris, whose appointment has expired.

MUNRO, A. B., M.D., C.M., has been appointed Medical Officer to the Medical Aid Association of Friendly Societies, Bradford, Yorkshire, vice Farrar, whose appointment has expired.

MURIEL, J. T., M.R.C.S.E., has been appointed Joint Medical Officer of Health for the Hadleigh Urban Sanitary District.

NEWMAN, H., M.R.C.S.E., has been appointed Joint Medical Officer of Health for the Hadleigh Urban Sanitary District.

PALFREY, J., M.D., M.R.C.P.L., has been appointed Senior Obstetric Physician to the London Hospital, in succession to Dr. Head.

SPICER, N. W., jun., M.R.C.S.E., has been appointed Medical Officer and Public Vaccinator for the Parish of Chardstock in the Axminster Union for one year.

STEVENSON, J., M.D., M.R.C.S.E., has been appointed Medical Officer of Health for the Parish of Paddington, vice Hardwicke, resigned: £300 per annum.

SWETE, E. H. W., M.D., M.R.C.S.E., has been appointed Medical Officer of Health for the Droitwich Rural and Urban Sanitary Districts: £150 per annum for two years; acreage 52,893; population 18,969.

TAYLOR, C. L., L.R.C.P.L., M.R.C.S.E., has been appointed House-Surgeon to the Wolverhampton and Staffordshire General Hospital, vice Appleyard, resigned.

WALLACE, Mr. E. J., has been appointed a House-Physician to the Western Infirmary, Glasgow (late House-Surgeon to the Royal Infirmary, Glasgow), vice C. Plumer, M.D., whose appointment has expired.

WALSHE, T. P., L.K.Q.C.P.I., F.R.C.S.I., has been appointed Medical Officer, Public Vaccinator, and Registrar of Births &c., for the Ballyroan Dispensary District of the Abbeyleix Union, Queen's County, vice O'Kelly, resigned.

WEBB, S. H., L.K.Q.C.P.I., L.R.C.S.I., L.M., has been appointed Resident Medical Officer to the Dover Hospital and Dispensary, vice Grandison, resigned.

YOUNG, A., B.A., L.S.A., has been appointed Resident Accoucheur to King's College Hospital.

Births, Marriages, and Deaths.

BIRTHS.

BOSTOCK.—On the 6th inst., at North Pallant, Chichester, the wife of A. S. Bostock, M.R.C.S.E., of a daughter.

BUCK.—On the 8th inst., at Camden-road, the wife of A. H. Buck, L.R.C.P.Ed., of a son.

GRAY.—On the 17th ult., in the Bay of Biscay, on board the S.S. "City of Poonah," bound for Calcutta, the wife of Surgeon H. A. Chatham Gray, M.B., of H.M.'s Indian Army, of a daughter.

GRIEVE.—On the 10th inst., at Crouch-end, the wife of Robt. Grieve, M.D., of a daughter.

REID.—On the 7th inst., at Canonbury-park North, the wife of Alex. Reid, M.R.C.S.E., of a son.

SAMSON.—On the 7th inst., at Duncan-terrace, City-road, the wife of A. E. Samson, M.D., of a son.

MARRIAGES.

M'ANDREW—EYRE.—On the 12th inst., at the Roman Catholic Church of Our Lady, St. John's-wood, James John M'Andrew, M.R.C.S.E., of Limehouse, formerly of Co. Mayo, to Teresa Mary Wilhelmina, third daughter of John Joseph Eyre, Esq., J.P., of Clifden Castle, Co. Galway, and Abbey-road.

ODELL—BAYLIS.—On the 27th ult., at St. John's Church, Notting-hill, Thos. Odell, Surgeon, of Hertford, to Hannah, elder daughter of John Baylis, Esq., of Bengeo, Hertford.

DEATHS.

CAMPBELL.—On the 25th ult., at Arundel-street, Strand, John Campbell, M.D., Surgeon R.N., aged 27.

HARRISON.—On the 6th inst., at Woodbine-street, Westoe, Colpitts Harrison, L.S.A.L., aged 64.

HAWKE.—On the 17th inst., C. J. Hawke, late Colonial Surgeon, St. Vincent, West Indies.

LARA.—On the 6th inst., Benj. Walters Lara, Esq., of Palsgrave-place, Temple, London (only son of Benj. Lara, M.D., F.R.C.P.E., late of Southsea, Hants), in his 53rd year.

M'DERMOTT.—On the 10th inst., John M'Dermott, M.D., of Shrewsbury-road, Westbourne-park, aged 78.

NEWMAN.—On the 4th inst., at New John-street West, Birmingham, A. S. Newman, M.R.C.S.E.

OFFICER.—On the 24th ult., at Fetteresso, Kincardineshire, Alex. Murray Officer, M.D., aged 55.

RIDLEY.—On the 3rd inst., John Ridley, M.D., of Moore Hall, Tullamore, aged 59.

SHARPE.—On the 3rd inst., at Wilton-street, Liverpool, Robt. Sharpe, M.D., aged 47.

[N.B.—*A fee of 5s. is charged for the insertion of Notices of Births, Marriages, and Deaths.*]

Notes, Short Comments, and Answers to Correspondents.

HOMŒOPATHY.

WE pretend to no excessive modesty; but a correspondent, who has addressed us on the subject of homœopathy, is a little more sanguine as to the effects of our articles than we are when he seems to expect them "to eradicate this plague-spot from our profession." Human nature is a very weak thing, even after having been subjected to a medical education. It is still more weak and impressible outside the profession. And we believe that if we wrote an article every week, ever so moderate and convincing, many would be of the same opinion still. The School Board will have to be a much older institution before quacks, in or out of the profession, will fail in finding believers. For all that, we agree with our correspondent that it is proper now and again to indicate the points which distinguish scientific medicine from homœopathy. It is but a short time since we held up to ridicule the doctrines of the founder of homœopathy. These doctrines are now so far abandoned that many of his followers would wish to obliterate the name of homœopath from the dictionary. They seem angry with us for using it; but we are not responsible for it, and they cannot get rid of it. Many of the followers of Hahnemann still stick to it, and, with a strange mixture of brass and innocence, put it on their brass-plates, Dr. So-and-so, *Homœopath*. It is not we who have separated the followers of Hahnemann. They have separated themselves. They have left the great host of plodding workers in therapeutics, and thought to arrive at knowledge and skill in curing disease, not by work and observation, but by adopting a theory the most absurd and improbable that was ever imagined. This theory is being contradicted every day by discoveries as to the curative action of medicines. It has always been contradicted by the common sense of the profession and of the public. Almost every week witnesses some new discovery of the power of one drug to cure the poisonous effect produced by another; but in every such case the medicines are antagonistic, and not homœopathic in their action. And so it is likely to be in regard to the cure of disease. But we are not going to fall into the error of the homœopaths, and amuse ourselves by constructing a theory. We must be content to observe facts. It is unexciting work. But the observer of one real fact is worth a hundred Hahnemanns, who on a baseless theory have accumulated an unprecedented number of false conclusions.

THE SAMARITAN INVALID BEDSTEAD APPARATUS.

SOME weeks ago a correspondent sought for information through our columns as to an invalid bed which could be raised to any angle. The result of the search would seem to imply the existence of a need for more effective appliances for the comfort of bedridden patients. We have recently had an opportunity of inspecting an apparatus constructed by Mr. Wm. Jack, of 118, Culford-road, E., which, from its ingenuity and completeness, is worthy of notice. By its means a sufferer, though confined to bed, may for a time be rendered almost independent of nurses and attendants. The apparatus consists of an iron framework and sacking, capable of being attached to an ordinary iron bedstead. By simply turning a handle on the right-hand side, the invalid may raise to any angle the head and shoulder portion of the apparatus, and at the same time depress the part on which the legs rest, so as to convert his recumbency into a sitting posture. The head board is fitted in such a manner that the patient may without difficulty bring it in front of him to serve as a firm table, to which, if desired, a reading desk may be readily affixed. Another important adjunct is the appliance for the use of the bed-pan. By manipulating a handle on the left hand of the bedstead, a pad slips from the centre of the mattress, and the commode is brought beneath the patient; again, it is removed and the pad restored to its former position by shifting the handle in the reverse direction. All this may be done by the invalid himself whilst lying down. Not the least valuable feature of the apparatus is the device by which the nurse, by pulling a strap, may gently turn the patient on to either side of the bed—an arrangement the advantage of which will be readily appreciated by anyone who has had much experience of the difficulties attending bedridden cases. One or two minor matters, though useful, in connexion with the apparatus, need not be specially mentioned.

Hopeless.—We never recommend a particular physician or surgeon.

THE GERMAN HOSPITAL, DALSTON.

To the Editor of THE LANCET.

SIR,—The columns of the principal daily journal of the 17th April report, under the above heading, that this hospital was honoured on the previous day by His Royal Highness the Prince of Wales presiding at its anniversary dinner. This kindly and condescending act on the part of His Royal Highness, even independent of the magnificent pecuniary result (£5000), is so encouraging to every well-wisher of that most useful institution that I, as one of them, cannot help remembering some incidents, from which it would appear that the very idea of such a hospital for London was coeval with the birth of His Royal Highness, and that his baptism exercised no small influence on that idea being successfully carried out.

In 1841 and 1842 I was greatly impressed with the desirability of such a hospital—nay, I felt sure there was a great want of it for the German poor in London. I frequently gave expression to this thought of mine in private, with more or less effect; but one Sunday (in 1842), on my seeing the Prince's sponsor, His Majesty the late King of Prussia, during divine service in St. Paul's Cathedral, I was at once attracted by the genial and kindly countenance of the King to select him as the one to whom I might with confidence communicate the resolution which I had formed of carrying my idea into effect. I wrote a letter intended for the King after his return to Berlin, in which I represented the whole object in view, with all the reasons for it; but I still hesitated to send it on until my kind and valued friend, the Rev. Mr. Sydone, Chaplain of His Majesty, then in London, forwarded it to the Minister of State, Von Mühler, at Berlin. His Majesty the King appears to have read the letter with all the warmth of his heart; for he immediately instructed his Excellency, the late Chevalier Bunsen, his Ambassador here, to put himself in communication with me on the subject. Upon that I addressed myself with like confidence to Her Most Gracious Majesty the Queen of England and to Her Royal Consort, who were both pleased to take a very great interest in the matter, and I had the distinguished honour of being personally introduced to his Royal Highness the Prince by the late Baron von Stockmar. I shall never forget the words of the Prince on that occasion, who advised me not to allow myself to be deterred by any difficulties that might arise, as the plan was too good ever to be given up; nor can I forget the immediate result of that interview, because I obtained between £500 and £600 in subscriptions on that day, the late Mr. Frederick Huth heading the list with £200. I after that asked the candid opinion of some of the most eminent English physicians and surgeons, who all expressed themselves highly in favour of the plan. Thus encouraged, I unremittingly worked on, first for a long while yet single-handed, and then with a Committee that was formed at my instigation, when the subscriptions already amounted to above £1300, and I did not rest till my original plan of an independent institution, with sanatorium and dispensaries, was carried out in all its details.

The hospital was opened under the auspices of H.R.H. the present Duke of Cambridge (then Prince George) on the 15th October, 1845, the birth-day of the late King of Prussia, and I had the good fortune of receiving the first patient into it, of giving it my services (having gratefully declined the offer of £300 per annum) as honorary directing physician during the first few difficult years of its existence, and of organising a wider sphere of its usefulness by the free admission of English applicants as out-patients to the hospital.

I remain, Sir, yours most respectfully,

J. C. H. FREUND, M.D.,
Late Deputy Inspector-General of Hospitals
(during the Crimean War).

London, May 6th, 1875.

OBSTETRIC CLAMPS.

Mr. J. W. Ayres (Greenwich) has forwarded for our inspection a pair of small obstetric clamps, which he appears to think will prove of service to obstetricians. They are neatly got up, and enclosed in a small, easily portable case, and it is quite conceivable that they should be found useful in certain complications which are met with in the practice of midwifery. They are made by Perry and Co., of Greenwich and London.

Dr. Fothergill.—Very soon.

MILTON'S NOSOLOGY AND ETIOLOGY.

It is interesting to note, in the ninth book of "Paradise Lost," Milton's views of the nature of disease and of the more common causes of death. They appear in the communication of the Archangel Michael to Adam, detailing the misery which the "inabstinence of Eve shall bring on men." Among the causes of death, intemperance was to hold a leading place:—

> "Some, as thou saw'st, by violent stroke shall die,
> By fire, flood, famine, by intemperance more
> In meats and drinks, which on the earth shall bring
> Diseases dire."

Then follows a vision of a lazar-house, in which a most ample *clinique* was collected. The nosological list is really, for inclusiveness and brevity, not unworthy of the attention of the College of Physicians. In this place—

> "Were laid
> Numbers of all diseased: all maladies
> Of ghastly spasm, or racking torture, qualms
> Of heartsick agony, all feverous kinds,
> Convulsions, epilepsies, fierce catarrhs,
> Intestine stone and ulcer, cholic pangs,
> Demoniac frenzy, moping melancholy,
> And moonstruck madness, pining atrophy,
> Marasmus, and wide wasting pestilence,
> Dropsies, and asthmas, and joint-racking rheums."

If Milton had lived in the winter of 1875, he could not have got a more apt adjective for describing catarrhs—"fierce catarrhs."

Dr. Burnett, (Biggleswade.)—The lecture by Dr. McCall Anderson in THE LANCET of October 24th, 1874.

THE TEMPORARY SCARLET-FEVER HOSPITAL AT CASTLE CARY, SOMERSET.

To the Editor of THE LANCET.

SIR,—In December last scarlet fever made its appearance in Castle Cary, after having been for some time in towns in the neighbourhood. Directly the fact became known, Mr. George Gray, a gentleman living in the place, suggested that a temporary hospital for isolation should be prepared, and offered to lend a house for the purpose. This house, however, could not be used in consequence of the opposition of the persons living in the next houses, and, after several other places had been considered, the rooms used for the infant school were obtained. The children had left because the mistress was ill with the fever. The school is healthily situated, and away from the main streets. A large room, about 14 feet by 32, and 18 to 20 feet high, was the chief ward; it held twelve patients and one nurse. The mistress's room became the nurses' room, and there were two bedrooms about 12 feet square, and some small offices. Mr. Gray, who, with Mrs. Gray's assistance, managed the affair from beginning to end, at once sought subscriptions, and with little difficulty got promises to the amount of £4 per week as long as the hospital was needed. Bedsteads, bedding, and cooking utensils were lent or given, and the necessary stores of provisions procured. A lady from a nursing institution at Warminster, Wilts, kindly gave her services as superintendent and head nurse, and the guardians of the union provided an under nurse. They also consented to pay 3*s.* 6*d.* per week for each patient received. Our hospital ambulance was an old wheel-chair, given for the purpose, and in it the first patients were brought in on Dec. 9th. The hospital was occupied from Dec. 9th, 1874, to March 17th, 1875, seventeen patients having been admitted. The average period of their stay was five weeks and one-third. No death occurred. Most of the cases ran a simple course—four or five were very ill; one was in a typhoid condition for several days; one had pericarditis; one had convulsions, &c. The patients varied in age from two to seventeen years. The medical care of the hospital devolved chiefly on Mr. W. Randall. The treatment consisted of warm sponging, a little carbolic acid being added to the water; beef-tea and milk for bad cases; full hospital diet; an ounce or two of wine for the convalescents and slighter cases; and quinine throughout the attack, except in albuminuria, when some form of iron was used instead. At the end of six weeks the first nurse was obliged to leave, and a successor was obtained from the East Grinstead Home. The ladies were provided with board and lodging, and their railway fares paid, and a collection on behalf of the institutions from which they came was made in the parish church. They both were most kind and skilful, and the success of the undertaking was largely due to them. As to expenditure and income—

Bedding, fitting up, cleaning &c. hospital	...	...	£9 5 10
Bread, milk, and other provisions	...	...	22 9 3
Beer and wine	...	...	3 17 0
Coal and gas	...	...	6 15 3
Washing	...	...	0 17 0
Disinfectants	...	...	1 4 0
Printing &c.	...	...	0 7 6
Wages, travelling expenses, and gift to under nurse			8 13 6
			£53 9 4

The money was obtained thus: £11 11*s.* from the union, and £41 18*s.* 4*d.* from the subscribers, the whole amount promised not being needed. In Castle Cary there were thirty-five cases of fever; the exact number in other places are not obtainable; but towns with nearly the same population have had three or four times as many cases, and the epidemic has lingered in them much longer than in Castle Cary. This favourable result is not attributed wholly to the hospital. A very thorough system of drainage made a few years ago may have prevented the spread of the epidemic. The decrease in the mortality from all causes since the sewage improvement has been very striking.

These notes have been published as a help to others who may need to establish a temporary fever hospital.

Yours truly,

Castle Cary, May, 1875. C. P. COOMBES, M.D. Lond.

THE LATE OUTBREAK OF TYPHOID FEVER AT CROSSHILL AND EAGLESHAM.

We have received copies of two very interesting reports, which we purpose noticing next week, containing an account of the outbreak of this disease, which adds another to the list of such occurrences that have been traced to the use of tainted milk.

THE LIFFEY NUISANCE.

As the terrible stench arising from this river has again attracted notice, owing to the warm weather, the Corporation of Dublin hit upon the ingenious method of trying to disarm criticism by attacking the Port and Docks Board, and accordingly summoned that body to compel them to keep in order the river, which they state is under that Board's jurisdiction, and does not belong to the Corporation of Dublin. The summons was heard on Wednesday, but was, we understand, dismissed. Neither body will stir in the necessary sanitary work of purifying the Liffey, and will not permit anyone else to move in the matter. Two members of the Corporation lately volunteered to undertake the expense of keeping it temporarily in a healthy condition, but their services were not accepted; whilst the proposal of Sir John Arnott, the proprietor of the *Irish Times*, who some time since guaranteed at his own cost to perform the same work, was also declined. This gigantic system of intercepting sewers, which will cost an enormous sum of money, and which cannot be completed for several years, may be all very well when finished; but that is no reason why something temporary should not in the meantime be done to remedy the present unsatisfactory condition of affairs.

Dr. G. Heath, (Newcastle-on-Tyne.)—The article has been received, and will be published at a very early date.

OBSTETRIC PRACTICE IN THE NORTH.

To the Editor of THE LANCET.

SIR,—In your publication of the 17th ult. there is an account of a strange practice in the north of England relative to women in labour. Allow me to notice a practice of the kind still farther north—namely, in the hill district of Banffshire. Practising in that district in the early part of my professional career I was called to attend a woman in labour. I found her kneeling on a pillow on the floor, with her knees a little separated, a female attendant kneeling each side of her, the patient having an arm round each neck. The labour progressed quickly. The child had to be supported in the hands of an assistant while I tied the cord. All went well; but the position was so inconvenient, as well as dangerous, that I endeavoured to put a stop to the practice. Being the only medical man in the district, I succeeded, but with some difficulty. The prejudice in its favour was so firmly rooted that nothing put a stop to it but my resolute refusal to attend unless the patients confined themselves to bed.

It may be well to consider whether something may not be said in favour of such positions. They have been, and I believe still are, practised in many places. In some parts of Germany to this day the accoucheur has a chair constructed for the purpose, which is sent before him to the house of the patient, on which she seats herself; and we know that the Israelites in Egypt had some contrivance of the kind called "stools," probably two stools tied together in front, and separated behind in the manner mentioned by your correspondent in the north of England. The position of a woman during parturition is certainly of importance, and there is no doubt that these unusual positions give to the patient considerable power in her expulsive efforts; and we can easily perceive that in lingering, and indeed in all natural labours they will expedite delivery, save time, the patient's strength, and, in some cases, the use of forceps. The practice of tying a cord to the foot of the bed, and giving it to the patient to hold during a pain, is often employed for this purpose; but it cannot be so effectual as the above-mentioned positions. The dangers of these, however, as injury to the child, rupture of the cord, premature separation of the placenta and hæmorrhage, inversion of the uterus, &c., will, in my opinion, far outweigh their advantages, and I would in lingering cases prefer the use of forceps, although they may in some cases be given a careful trial before having recourse to instruments, particularly when the patients or their friends have great aversion to their use.

Your obedient servant,

Kingussie, Inverness-shire, May, 1875. J. C. ORCHARD, M.R.C.S.

To the Editor of THE LANCET.

SIR,—I can fully endorse the opinion expressed in "L.R.C.P.'s" letter in your impression of the 17th ult., in which he speaks of the disgusting custom of the country in regard to midwifery practice, which permits the patient to sit on two chairs, which are separated behind, and through which aperture the accoucheur has to manipulate. It is clearly dangerous to both mother and child, and I think degrading to the medical man, so much so that I wonder it is at all tolerated, although I am aware that many medical men in large practices amongst the Durham and Northumberland collieries have no objection to it. My only experience of it was a dead failure. I was very young, only a second-year student (and young for that), when amongst my first cases I unfortunately allowed a patient, at her own and friends' request, to adopt the above position. I remember blushing at never having been taught, what all the women in the house said, was best! No sooner was the patient seated than a strong pain came on, and a large *ten-pounder* was, as it were, *shot* upon the brick-floor, gave two gasps, and died! I have kept this a secret for many long years, and I now make it a present to the "young doctors" of the colliery districts.

Amongst the lower Irish classes in Glasgow it is a common practice for the woman to be confined resting her hands and knees upon the floor, and without taking her clothes off until after she is delivered. In this case the accoucheur, of course, manipulates from behind. I do not know whether this is a common Irish custom; but, although perhaps less dangerous, it is equally dirty and disgusting.

Yours &c.,

Portland, May 7th, 1875. R. N.

SURFACE WELLS.

DR. WHITMORE, medical officer of health for Marylebone, reports that last month he analysed samples of water from the surface wells in the parish, now open for the public, and found that all were unfit for drinking purposes. Each sample examined contained a large quantity of decomposing vegetable matters, together with living organisms. The obvious recommendation is to close up the wells without delay.

LIVERPOOL WATERCLOSETS.

SOME of our Liverpool contemporaries are much exercised with the discovery that there are in Liverpool watercloseets directly connected with the water-mains. Not unnaturally, this being the case, they protest against such a state of things being left unremedied one moment longer than necessary, and the subjection of portions of the population to the liability of poisoning by excrement or sewer-air sucked up into the water-mains during interruptions of supply. Their alarm will not be diminished by reading the reports of Dr. Blaxall and Dr. Buchanan on outbreaks of enteric fever produced in this manner. These reports are published in the Supplementary Report of the Medical Officer of the Local Government Board just issued, and they prove conclusively the imminence of the danger arising from watercloseets connected directly with the water-mains.

RUMSEY TESTIMONIAL FUND.

THE following additional subscriptions have been received on behalf of the above:—

Total acknowledged on 1st inst. ... £457 13 0

Rt. Hon. Earl Derby ...	£10	10	0	G. H. Pinckard, Esq.... ...	£5	5	0
Richd. Quain, M.D., F.R.S.	10	10	0	T. Stevenson, M.D., F.C.S.	1	1	0
George Graham, Esq.... ...	5	5	0	Robt. Elliot, M.D.	0	10	6
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Newcastle-on-Tyne	„	G. H. Philipson, M.A., M.D, 7, Eldon-square.

Scarlatina.—It is impossible to give a definite answer to our correspondent. Everything turns upon the nature of the case. She had better consult some aural surgeon.

EXPERIENCES OF BRIGHTON DRAINAGE.

To the Editor of THE LANCET.

SIR,—As the town in which I reside was visited by typhoid fever, it was thought desirable that my children should not return from school to it. I therefore took up my quarters at Brighton. I had not, however, been long in the house I engaged before I perceived the drains were out of order. Happily I had only to mention the fact, and at once the drain was examined, and found to be stopped up by a piece of an old waistcoat. This appeared to be a satisfactory solution of our annoyance; but after a few days the smell returned worse than ever. A builder was called in, and the drain thoroughly examined, when it was found that the stoppage was in the street. The town authorities were then communicated with, and without delay a shaft was sunk in the street, and everything put straight. The stench, however, till the work was completed, notwithstanding the use of disinfectants, was, as might be expected, very hard to bear, and might have been very serious.

And now for the cause of the mischief, which will, I am sure, be hardly credited, but which I am able to give on the authority of a person—I presume one from the surveyor's office—who was superintending the work.

On examining the connexion of the house drain, it was found that it was not connected with the new town drain, but actually ran *into an old brick drain, which was disused*, and which, in fact, was nothing better than an elongated cesspool. I must confess this so excited my surprise that I stated I could hardly believe such a thing to be possible, and that I thought the town authorities ought to see that all houses were properly connected with the main drain, more especially lodging-houses; to which the reply was vouchsafed "that the town authorities knew nothing of the drainage of the houses prior to the time of the Corporation taking over the government of the town."

Now surely, Mr. Editor, this is most reprehensible. The town authorities ought to know the drainage of every house, and to take steps that all houses should be connected with the main sewer. If they neglect this duty, what avails a grand drainage scheme, when visitors, who cannot protect themselves, are liable to be poisoned by the ignorance or cupidity of landlords? In my own case I wish to do my landlord full justice He supposed his house-drain was connected with the sewer, and never for one moment dreamt that his drainage was only into a pestilential bricked ditch. I hardly know how to use any name strong enough, and, as the town authorities knew nothing, the ignorance of the landlord is not much to be wondered at. It would be very interesting to know, if the town authorities would vouchsafe the information, what percentage of houses they know for certain are connected with the main sewer, what number they know for certain are not connected, and what number they are wholly ignorant of.

In the years 1867-68 I resided in the upper part of Brighton, where there was at that time no other drainage than by means of cesspools; the fine new houses in the upper part of Denmark-terrace were being built, and I observed a large well being sunk in the chalk in the little piece of frontage garden. I knew the well could not be for water, and therefore inquired the object, when, to my surprise, I was informed it was for the purpose of a cesspool. Since that time the authorities have proceeded most vigorously with the drainage scheme; but the questions I have propounded above are most important, and I should like, and so I am sure would all visitors, to know that steps will be taken to do away with all cesspools or drainage other than that into the main sewer.

Yours obediently,
May, 1875. A SUSSEX RECTOR.

R. W. Macdonald, M.D. Erlangen, &c., treats our article on Degrees for Practitioners with scant justice, not to say with ungenerosity. It was not our intention to apply reproachful epithets to foreign graduates. The whole gist of our article was to the effect that many very respectable and meritorious practitioners are obliged to go abroad for degrees because of our home system being adapted only to those who are fresh from the schools. Dr. Macdonald will do well, in writing in defence of foreign degrees, to write with more moderation.

THE INDIAN MEDICAL SERVICE.

To the Editor of THE LANCET.

SIR,—Will you allow me space for a few remarks on some facts connected with the Indian medical service, which are intended chiefly for the information of those who may have intentions of entering this service.

In the memorandum on the pay and position of medical officers in the Indian army, issued by the Government for the information of candidates for the service, it is stated that surgeons under five years' service, who have passed the "lower standard" examination in Hindustani, will receive pay at the rate of 450 rupees per mensem when appointed to take charge of a native regiment. It is on this understanding that nine-tenths of those who enter the service go out to India, and certainly the wording of the memorandum above alluded to would not admit of a doubt on the point to any uninitiated reader. Strange to say, however, the conditions, apparently so clearly laid down, are not adhered to, and the young surgeon when appointed to take charge of a native regiment is told (if he have been bold enough to complain), that as he is only *officiating* in charge, he is in consequence not entitled to the full pay of the appointment until he receives it permanently, which is not usually the case nowadays until he has been in the service four or five years. This arrangement is perfectly well understood in India; but candidates are, with very rare exceptions, in complete ignorance of it; consequently on arrival in India they are compelled to come to the very disagreeable conclusion that the emoluments of the service they have entered are much lower than they had been led to believe from the Government circular.

I am confident I am expressing the opinion of every officer in the Indian medical service in saying that it would be much more creditable to the Indian Government, and infinitely more satisfactory to young surgeons entering the service, if the information given to candidates as to the amount of pay they will receive during the first few years of their service were more explicit and detailed.

In concluding, I will add a word of advice to those candidates who may be aspiring to the honour of a high "place" at the entrance examination, and this advice is: Do not indulge any hopes of reward for your labour, however much they may be fostered by those in authority at Netley; the fact of having taken a high position at the entrance confers no advantages beyond choice of Presidency and seniority in the Army List. The first on the list has exactly the same chance of a good appointment as the last, or it may be not so good; since the latter may be fortunate enough to possess "interest," which is the philosopher's stone of India.

It is with the object of preventing future medical "griffs" from experiencing the disappointment with which I and a host of others have met on first arrival in India that this letter is written.

I remain, Sir, yours obediently,
March 19th, 1875. MEDICUS.

THE LATE CHARLES LEVER.

THE favourite writing-table and arm-chair of this well-known writer have been presented to the University of Dublin by his daughter, the table bearing the following inscription:—

Writing table of the late
Charles James Lever, LL.D.,
Obit. June, 1872.
Presented to Trinity College, Dublin,
by his Daughter,
Julia Kate Neville,
1874.

Mr. T. A. Handsley.—The charge per visit should be in accordance with previous charges, and with the custom of the place, unless the visits in point of length or time implied an unusual inconvenience to the practitioner.

URINARY DEPOSITS.

To the Editor of THE LANCET.

SIR,—Will you permit me to notice the communication of "Senex" in your issue of the 1st inst. He finds difficulty in accepting, in the face of personal experience, that which reflection should have led him to anticipate, and is careful to qualify an inference for which there is ample evidence before the profession. From the days of Prof. Oertel, who claimed for water-drinking the dignity of "cure-all," down to our own times, when we have the united testimonies of Drs. Prout, Beale, Harley, and J. C. Murray to its salutary influence in the removal of the conditions attended with urinary and blood deposits, such as phosphates, urates of ammonia and soda, and effete matter generally, there has been a gradually growing prepossession in favour of the practice. "Hydropathists" have from the first employed it with advantage, though frequently pushing it to absurd and injurious lengths. To a certain extent its action in lithiasis is deceptive, as, through increasing the relative proportion of water to salts in the urine, it diminishes the tendency to deposition in a visible form, and so far hides rather than removes the evidence of malassimilation. But there is a real and substantial benefit through the diminution it effects as a diluent in the irritating qualities of the urine, and a still greater benefit is realised in the flushing and cooling of the congested liver, which is usually associated with the state denominated lithiasis, and in a large proportion of cases stands in the relation of cause. I have rarely found any difficulty in obtaining the results which surprised "Senex," and trust his contribution may be the means of directing attention yet more widely to a practice of considerable utility, and in assisting the profession to arrive at a juster appreciation of the part played by the *water* as opposed to the *minerals* in the fashionable "spa" practice of the present day. When a patient is projecting a trip to Carlsbad or Vichy, might not the medical adviser direct his attention to the domestic pump or water-tap, with the remark that "he might go farther and fare worse."

Yours &c.,
Matlock, May, 1875. W. B. HUNTER.

A Subscriber.—The "Almanach Général de Médecine pour la France, l'Algérie, et les Colonies, 1875," to be obtained of Williams and Norgate, Henrietta-street, Covent-garden, contains all the information desired.

Canadian.—It cannot be registered in this country.

THE FAMILY OF THE LATE MR. THOMAS.

To the Editor of THE LANCET.

SIR,—The following additional subscriptions have been received on behalf of the above:—

J. W. Barry, Esq., Ramsgate	£1	1	0
Mrs. Hooper, Burton-on-Trent	2	0	0
Misses Jackson, Tutbury	1	0	0

Yours truly,

FREDK. WILKINS, Treasurer.

Barwood Hill, Burton-on-Trent, May 11th, 1875.

COMMUNICATIONS not noticed in the current number will receive attention in our issue of the ensuing week.

COMMUNICATIONS, LETTERS, &c., have been received from—Prof. Humphry, Cambridge; Dr. Robert Barnes, London; Mr. Haynes Walton, London; Dr. Hughlings Jackson, London; Mr. Bader, London; Dr. Bastian, London; Mr. Hulke, London; Dr. Buzzard, London; Dr. Lyon Playfair, London; Dr. Ralfe, London; Dr. Palfrey, London; Mr. Solomon, Birmingham; Sir C. Trevelyan, London; Mr. Clarke, Dalton-in-Furness; Mr. A. Duncan, London; Dr. Daly, London; Dr. Chalmers, London; Rev. Mr. Hatchard, Hastings; Mr. Howse, London; Dr. Lowe, Lincoln; Mr. Meadows, Hastings; Mr. Teevan, London; Dr. Fothergill, London; Dr. Corfield, London; Mr. Ayres, London; Mr. R. Nelson; Mr. Peacock, Scarborough; Dr. Burnett, Biggleswade; Mr. J. Dryden; Dr. Arnison, Newcastle-on-Tyne; Dr. Berry, Wigan; Mr. Sewill, London; Mr. Blackett, London; Dr. Fleming, Glasgow; Mr. Bott, Huddersfield; Dr. Vacher, Birkenhead; Mr. H. F. Barnes; Dr. Hime, Sheffield; Dr. Goldie, Leeds; Mr. Bailey, London; Mr. Larkin, London; Mr. Oakes, Birmingham; Mr. Addenbrooke, Kidderminster; Mr. Thompson, Pleasington; Dr. Carr, Blackheath; Mr. Sherlock, London; Mr. Bond; Mr. Orchard, Kingussie; Mr. Fowler, London; Dr. M'Donald, Routh; Mr. Wiglesworth, Liverpool; Mr. Foster, London; Mr. Courtenay, London; Dr. Evans, Cardiff; Dr. Keith, Edinburgh; Mr. Wilkins, Burton-on-Trent; Dr. Hamerton, Mansfield; Mr. Fife, Norwich; Mr. Atkinson, Ripponden; Mr. Hemsted, London; Dr. Powell, London; Dr. Newman, St. Martin's; Mr. Hobson, Great Berkhamstead; Mr. Hartley, Cheltenham; Mr. Groves, London; Mr. Renton, Knaresborough; Dr. Heath, Newcastle-on-Tyne; Mr. Kemp, Bournemouth; Mr. Fletcher, Broughton; Mr. Brown, Tudhoe; Dr. Tay, London; Dr. Lownds, Egham Hill; Mr. Maclehose, Glasgow; Mr. Pelton, Tunbridge Wells; Mr. Kimpton, London; Mr. Lipscomb, St. Albans; Mr. Wallace, Glasgow; Mr. Messer, London; Dr. Buchanan, London; Mr. Dane, Finchley; Mr. Job, London; Dr. Duncan, London; Mr. Ensor, Port Elizabeth, South Africa; Mr. Holloway, London; Mr. Malcolm, Wiesbaden; Messrs. Poole and Stacey, London; Mr. Dove, Stowmarket; Mr. Spearing, Great Marlow; Messrs. Ferris and Co., Bristol; Mr. Riddle, London; Dr. Freund, London; Dr. Hardwicke, Sheffield; Dr. Affleck, Edinburgh; Mr. Carolan, London; Mr. Etheridge, Teignmouth; Dr. Fox, Broughton; Mr. Rose, Stornoway; Dr. Gill, Folkestone; Dr. Arnold, Belfast; Dr. Nunn, Bournemouth; Rev. A. J. D'Orsey, London; Protector; The Director-General, Naval Medical Department; C.; A Medical Man; Secretary, Royal Institution; R. N.; F. G. L.; A Subscriber; Justitia; An Old Foundation Scholar; Hopeless; &c. &c.

LETTERS, each *with enclosure*, are also acknowledged from — Mr. Adams, Edinburgh; Mr. Smith, Burton-on-Trent; Mr. Robertson, Melbourne; Dr. Hunt, Melbourne; Mr. Griffiths, Bideford; Mr. Hamilton, Cambridge; Mr. Palen, Wrexham; Messrs. Roberts and Co., London; Dr. Akerman, London; Messrs. Puttick and Co., London; Mr. Jenkins, Holborn; Mr. Prebble, Bognor; Mr. Dempsey, Oldbury; Mr. Evans, Walthamstow; Mr. Chapman, Putney; Dr. Smith, Sedgefield; Mr. Whitaker, Derby; Dr. Knox, Bootle; Mr. Franklin, Newcastle; Mr. Reilly, New Cross; Mr. Istance, Risca; Mr. Denton, Leicester; Mr. Latley, Willingham; Mr. Odell, Hertford; Mr. Stuckey, London; Mr. Marsdin, Eastbourne; Dr. Tosswill, Exeter; Mr. Armistead, Paddington; Mr. Hamilton, Preston; Mr. Willmore, Stockbridge; Mr. Robbins, Dasfeld; Mr. Sale, Lancaster; Dr. Anderson, Lofthouse; Mr. Yeo, Dublin; Mr. Wright, Bury St. Edmunds; Mr. Buller, South Brent; Dr. Mason, Burton-on-Trent; Mr. Knight, Northampton; Mr. Sampson, York; Mr. Kilburn, West Auckland; Mr. Scott, Great Wakering; Mr. Brend, Birmingham; Mr. Brett, Bridlington; Dr. Wallich, Herne Bay; Mr. Wheeler, Manchester; Mr. Caird, Kilmuir; Messrs. Wheatcroft, Chesterfield; Messrs. Underhill, Wolverhampton; Mr. Maclean, London; Dr. Richardson, Bryngwy; Mr. Mason, Carlisle; Mr. Coleman, Armley; Mr. Dowker, Delmsley; Mr. Homeles, Carmarthen; Mr. Clarke, Lynton; Mr. Whalley, Bradford; Mr. Brown, Dorchester; Mr. Marsh, Hammersmith; Mr. Evans, West Bromwich; Dr. Crowther, Luddenfoot; Mr. Praen, Fettercairn; Mr. Morris, Swansea; Mr. Matchon, Bury St. Edmunds; Messrs. A. and W. Fox, Manchester; Dr. Edis, Crowle; Mr. Pearse, Dawlish; Mr. Miller, London; Mr. Lewis, Basingstoke; Medicus, Lochwinnoch; Alpha, Cardiff; B. A. Middlesborough.

Western Morning News, Welshman, Newcastle Daily Chronicle, Cork Constitution, Isle of Man Times, Huddersfield Chronicle, Manchester Guardian, Liverpool Post, Local Government Chronicle, Metropolitan, Society, Surrey Advertiser, Porcupine, Rugby Gazette, Newcastle Critic, Record, and *Leeds Mercury* have been received.

METEOROLOGICAL READINGS

(*Taken by Stewart's Instruments*).

THE LANCET OFFICE, MAY 13TH, 1875.

Date.	Barometer reduced to Sea Level, and 32° F.	Direction of Wind.	Wet Bulb.	Dry Bulb	Solar Radia. in Vacuo	Max. Temp. Shade	Min. Temp.	Rainfall.	Remarks at 8 A.M.
May 7	29·59	W S W	49	52	66	58	48	0·12	Cloudy
„ 8	29·83	W.	55	57	90	63	51	0·29	Cloudy
„ 9	29·89	S.W.	53	60	103	66	50	0·04	Fine
„ 10	30·09	W.	53	56	103	67	51	...	Overcast
„ 11	30·42	S.W.	48	53	103	70	46	...	Cloudy
„ 12	30·35	W.	50	55	109	75	49	...	Fine
„ 13	30·30	W.	55	61	...	...	53	...	Fine

Medical Diary for the ensuing Week.

Monday, May 17.

ROYAL LONDON OPHTHALMIC HOSPITAL, MOORFIELDS.—Operations, 10½ A.M. each day, and at the same hour.
ROYAL WESTMINSTER OPHTHALMIC HOSPITAL.—Operations, 1½ P.M. each day, and at the same hour.
ST. MARK'S HOSPITAL.—Operations, 9 A.M. and 2 P.M.
METROPOLITAN FREE HOSPITAL.—Operations, 2 P.M.

Tuesday, May 18.

GUY'S HOSPITAL.—Operations, 1½ P.M., and on Friday at the same hour.
WESTMINSTER HOSPITAL.—Operations, 2 P.M.
NATIONAL ORTHOPÆDIC HOSPITAL.—Operations, 2 P.M.
WEST LONDON HOSPITAL.—Operations, 3 P.M.
ROYAL INSTITUTION.—3 P.M. Prof. Gladstone, "On Chemical Force."
PATHOLOGICAL SOCIETY OF LONDON.—8½ P.M. The following Specimens will be exhibited:—Blood-cyst formed in Sarcoma; Carcinoma of Ovary; Worm in Right Ventricle of Heart of a Dog; Recent Specimens of Tubercle in Pheasants; Cancer in Common Fowl; Brain and Cord of Fowl; Softening of Cerebellum; Purulent Pericarditis; Cavity in Apex of Lung filled with Bony Deposit; A Fatty Tumour; Dry Gangrene of Lower Extremities; Prolapsus Linguæ; Hæmorrhagic Periostitis; Tumour in Sacral Region; Large Foramen Ovale without Cyanosis; Dislocation of Astragalus with Fracture; Parts from a Case of Cancer of Rectum two years after Colotomy; Melanotic Tumour of the Eye; Casts of Teeth from a Patient with Lamellar Cataract; Stricture of Colon plugged by a Gall-stone; Contracted Aorta (two Cases); Spontaneously Cured Aortic Aneurism; Hernia Testis in an Infant; Organs from a Case of Acute Tuberculosis; &c. &c.

Wednesday, May 19.

MIDDLESEX HOSPITAL.—Operations, 1 P.M.
ST. MARY'S HOSPITAL.—Operations, 1½ P.M.
ST. BARTHOLOMEW'S HOSPITAL.—Operations, 1½ P.M., and on Saturday at the same hour.
ST. THOMAS'S HOSPITAL.—Operations, 1½ P.M., and on Saturday at the same hour.
KING'S COLLEGE HOSPITAL.—Operations, 2 P.M., and on Saturday at 1½ P.M.
GREAT NORTHERN HOSPITAL.—Operations, 2 P.M.
UNIVERSITY COLLEGE HOSPITAL.—Operations, 2 P.M., and on Saturday at the same hour.
LONDON HOSPITAL.—Operations, 2 P.M.
SAMARITAN FREE HOSPITAL FOR WOMEN AND CHILDREN.—Operations, 2½ P.M.

Thursday, May 20.

ST. GEORGE'S HOSPITAL.—Operations, 1 P.M.
ROYAL ORTHOPÆDIC HOSPITAL.—Operations, 2 P.M.
CENTRAL LONDON OPHTHALMIC HOSPITAL.—Operations, 2 P.M., and on Friday at the same hour.
ROYAL INSTITUTION.—3 P.M. Prof. Dewar, "On the Progress of Physico-Chemical Inquiry."
HARVEIAN SOCIETY OF LONDON.—8 P.M. Dr. Mahomed: "A Contribution to the Clinical History of Scarlatinal Convalescence."

Friday, May 21.

ST. GEORGE'S HOSPITAL.—Ophthalmic Operations, 1½ P.M.
ROYAL SOUTH LONDON OPHTHALMIC HOSPITAL.—Operations, 2 P.M.
ROYAL INSTITUTION.—8 P.M. Evening Meeting.—9 P.M. Mr. J. B. Hamilton, "On the Application of Wind to Stringed Instruments."
MEDICAL MICROSCOPICAL SOCIETY.—8 P.M. Mr. W. B. Kesteven, "On Fatty Degeneration of Muscle."

Saturday, May 22.

ROYAL FREE HOSPITAL.—Operations, 2 P.M.
CHARING-CROSS HOSPITAL.—Operations, 2 P.M.
ROYAL INSTITUTION.—3 P.M. Mr. W. Herries Pollock, "On the Drama."

TERMS FOR ADVERTISING IN THE LANCET.

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For every additional line	0	0	6	For a page	5	0	0

The average number of words in each line is eleven.

Advertisements (to ensure insertion the same week) should be delivered at the Office not later than Wednesday; those from the country must be accompanied by a remittance.

N.B.—All letters relating to Subscriptions or Advertisements should be addressed to the Publisher.

Agent for the Advertising Department in France—

Mons. DE LOMINIE, 208, Rue Grenelle St. Germain, Paris.